NEW SERIES, NO. 150 (VOL. XX) JUNE, 1925

JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION

Formerly the Quarterly Publication of the American Statistical Association

CONTENTS

THE MORTALITY IN GERMANY 1913–1921. By E. Roesle 163

OUR UNSTABLE DOLLAR AND THE SO-CALLED BUSINESS CYCLE. By Irving Fisher 179

ON THE MEASUREMENT OF SEASONAL VARIATION. By O. Gressens 203

THE LEAST SQUARES CRITERION OF TREND LINES. By W. L. Crum 211

THE LAW OF DEMAND AND THE THEORY OF PROBABILITY. By Theodore Brown 223

NOTES:

The Refinement of Time Series. By Olin Ingraham 231

Price Changes in China. By J. Lossing Buck 238

An Extension of the Method of Moments (242); Forecasting Security Prices (244); The Cost of Living Indices of the N. I. C. B. and the B. of L. S. (249); On the Standard Error of Estimate (253); "Germany's Capacity to Pay" (256); The Progress of the Census (261); Miscellaneous Notes (264).

REVIEWS: Morley: *Unemployment Insurance in Great Britain*, by I. M. Rubinov (275); Rietz: *Handbook of Mathematical Statistics*, Forsyth: *An Introduction to the Mathematical Analysis of Statistics*, and Kent: *Elements of Statistics*, by K. G. Karsten (280); Forsyth: *Mathematical Theory of Life Insurance*, by P. H. Evans (285); Riegel: *Elements of Business Statistics*, by F. E. Richter (287); Giddings: *The Scientific Study of Human Society*, by F. H. Hankins (288); Smith: *Graphic Statistics in Management*, by R. Von Huhn (290); Moulton: *The Reparation Plan*, by F. D. Graham (294); Mills: *Statistical Methods Applied to Business*, by M. S. Applegate (295); Jerome: *Statistical Method*, by P. R. Rider (297).

RECENT LITERATURE 300

Published quarterly by the American Statistical Association

Rumford Press Building, Concord, N. H.

Editorial Office, New York City

Entered at the post-office, Concord, N. H., as second-class mail matter

Acceptance for mailing at the special rate of postage provided for in Section No. 1103, Act of October 3, 1917, authorized April 29, 1922

Officers of the American Statistical Association

ORGANIZED NOVEMBER 27, 1839

President, Robert E. Chaddock

Vice-Presidents, Edwin F. Gay
Henry L. Rietz
Walter W. Stewart

Counsellors, Louis I. Dublin
Joseph E. Pogue
Fred G. Tryon

Secretary-Treasurer, Willford I. King,
474 West 24th Street, New York City

Assistant Secretaries, James A. Field,
University of Chicago, Chicago, Ill.
A. J. Hettinger, Jr.,
31 Laurence Hall, Cambridge, Mass.

District Secretaries, Hornell Hart,
Bryn Mawr College, Bryn Mawr, Pa.
Dorsey W. Hyde, Jr.,
National Civics Bureau Chamber of Commerce,
Washington, D. C.

Editor, William F. Ogburn,
Columbia University, New York City

Associate Editors, Edmund E. Day
James W. Glover
Wesley C. Mitchell
Frank Alexander Ross
Leo Wolman,
465 West 23rd Street, New York City

Librarian, Horace L. Wheeler

Committee on Finance, Edmund E. Lincoln, *Chairman*
F. Leslie Hayford
Willford I. King

NEW SERIES, NO. 149 (VOL. XX) JUNE, 1925

JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION

Formerly the Quarterly Publication of the American Statistical Association

THE MORTALITY IN GERMANY, 1913–1921

THE EFFECTS OF WAR CASUALITIES AND FAMINE ON MORTALITY

By E. Roesle, M.D., *Member of the Board of Public Health, Berlin*

For those who want to investigate the mortality of a country, the knowledge of the death-rate of the various ages of each sex is indispensable. This means the government must provide statistics of population by age and sex. For mortality statistics require such records for each year, *i. e.* even for those years when no census was taken. The German board of statistics[1] accomplished this task by estimating the figures of population after the census of 1910 according to sex and years of birth, by adding to these figures the number of living-births, and subtracting the number of deaths and the number of emigrants to non-European countries which occurred in each subsequent year. This procedure of extrapolation is the most accurate method for estimating the population for those periods, when its natural increase is subjected to considerable changes, as is generally the case in years of war. The consequences of internal migration, of course, are not relevant, because the estimation according to age and sex is made only for the total of the population of the "Reich." Only a few countries have undertaken this task; most countries have confined themselves to indicating the estimated mean population, or, at best, to distributing it according to sex. A special difficulty arose for Germany from the diminution of its population caused by the numerous losses and changes of territories which occurred from 1919–1922. For this reason the absolute numbers of deaths covering that period are useless for time comparisons.

[1] *Statistik des Deutschen Reichs*, Vol. 276. Berlin, 1922, p. xlvii.

MORTALITY IN GERMANY DURING THE YEARS 1913–1921.
DEATH RATES ACCORDING TO AGE AND SEX PER 1000 LIVING AT EACH AGE-GROUP.

I. Mortality of the Children Between 0 and 15 Years.

Logarithmic scale

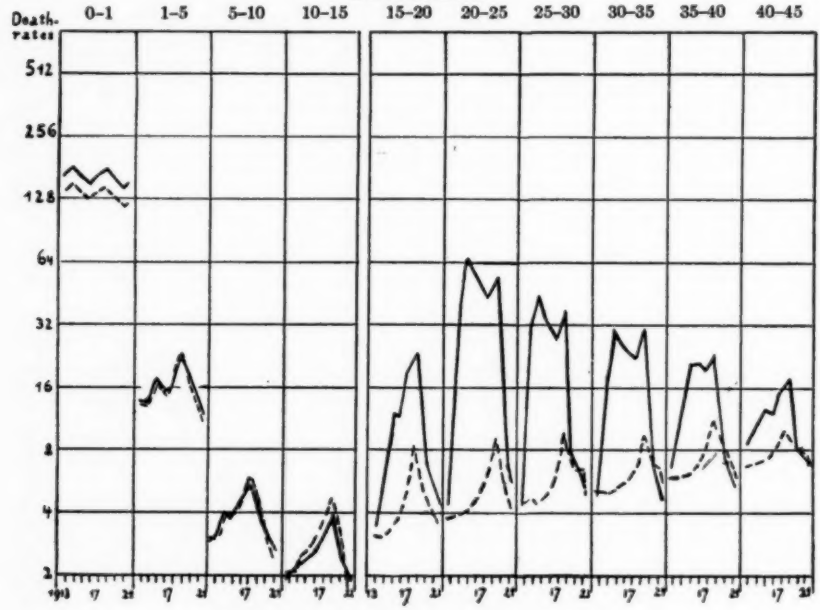
Effect of the famine during the years 1917 and 1918, and of the pandemic influenza in 1918.

II. Mortality at Age Between 15 and 45 Years.

Effect of the losses of life caused by war and of the pandemic influenza in 1918.

——— = Males

AGE-GROUPS:

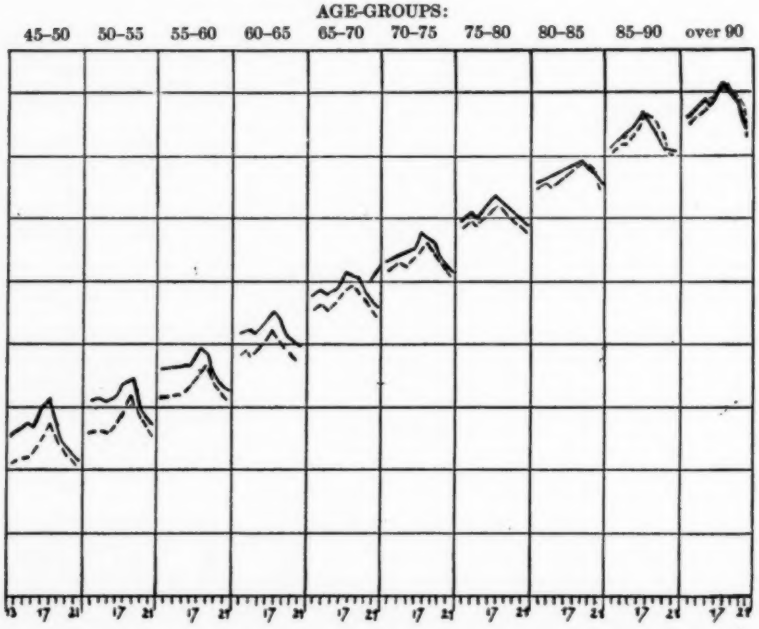


III. Mortality at Age of More Than 45 Years.

Effect of the famine during the years 1917 and 1918.

——— = Females

AGE-GROUPS:



I. MORTALITY OF THE TOTAL OF THE GERMAN POPULATION ACCORDING TO SEX

As to the death-rate of each sex, a considerable increase in all the war years is noted only for the male sex.

TABLE I

MORTALITY IN GERMANY ACCORDING TO SEX DURING THE YEARS 1913–1922

Year	Death-rate per 1,000 living of each sex and of both sexes			Proportion of the male death-rate to the female death-rate, the latter being stated as 100
	Males	Females	Both sexes	
1913	15.7	14.3	15.0	110
1914	23.4	14.8	19.1	158
1915	28.6	14.5	21.4	197
1916	24.7	14.1	19.2	175
1917	25.6	15.8	20.5	162
1918	30.4	19.5	24.7	166
1919	15.9	15.2	15.6	105
1920	15.8	14.7	15.1	108
1921	14.6	13.3	13.9	110
1922	15.0	13.8	14.3	109
1923	....	....	13.9	...

The year 1913 is a convenient basis for the computation of the increase of mortality during the war, as in that year mortality in Germany, as in almost all the other European countries, had reached its pre-war minimum. For in 1913 the male death-rate numbered 15.7, so that the death-rate of the female sex, amounting to 14.3, was surpassed by 10 per cent. It is a well-known fact that everywhere the mortality of the male sex is higher than that of the female sex. In Germany, this excess was almost stationary from 1872 to 1913. For in those years the male death-rate exceeded the female death-rate by 10–13 per cent almost regularly.

It goes without saying that during the war this excess increased very much. The difference is most marked for 1915, owing to the immense number of war casualties which, amounting to 97 per cent, were highest in that year; *i. e.* in 1915 the mortality of the male sex was nearly double that of the female sex. After its maximum in 1915, the male death-rate decreased considerably in 1916. From this year it rose again until 1918, so as to reach in the latter year a degree which surpassed even that of 1915. Since the end of the war, the mortality of the male sex has declined so greatly that the male death-rate of 1921 was lower even than that of 1913.

In the case of females, their mortality increased only in 1917 and 1918. The death-rate of the female sex for 1921 was likewise less than the minimum reached before the war. These statements, however, can by no means serve as a basis for the computation of the mor-

tality of the German civilian population during the war, for the decline of the crude death-rate in the war years was primarily produced by the heavy decrease in births noticeable since 1915. This decrease in births, of course, produced a progressive decline in the proportion which the deaths among infants and children represent of the total of deaths. The importance of this proportion is revealed by the fact that in 1913 there occurred 4.1 cases of death per 1,000 of the total population among the infants up to one year of age, and also that in this year the deaths of infants up to one year of age amounted to 27.6 per cent of the total deaths. In 1917 and 1918 there occurred only 2.2. deaths in this age group per 1,000 of the total population of *i. e.* 10.5 per cent and 8.8 per cent respectively of the total deaths. In spite of the marked decrease of deaths among infants up to one year of age, the total deaths in the war years rose almost uninterruptedly, so the number of deaths among the other age groups must necessarily have increased in a larger degree than the number of deaths among infants up to one year declined.

To demonstrate the influence which the decline of births during war time exerted on the course of mortality, I have compiled in the following table the necessary data for the female sex and for the same territorial unit, namely the former territory of the German Reich without Alsace-Lorraine from 1913–1918.

TABLE II

NUMBER OF FEMALE BIRTHS AND OF FEMALE DEATHS IN GERMANY (WITHOUT ALSACE-LORRAINE) DURING THE YEARS 1913–1918

Year	Number of female living-births	Difference as compared with 1913	Number of female deaths	Difference as compared with 1913
1913	873,372	.....	471,549	.....
1914	863,769	− 9,603	494,842	+ 23,293
1915	658,730	−214,642	488,776	+ 17,227
1916	488,085	−385,287	473,786	+ 2,237
1917	440,812	−432,560	530,690	+ 59,141
1918	447,161	−426,211	652,879	+181,330
Total	.....	*−1,468,503*	.....	*+283,228*

Year	Number of deaths among the female population of less than one year of age	Difference as compared with 1913	Number of deaths among the female population of more than one year of age	Difference as compared with 1913
1913	119,705	.....	351,844	.....
1914	129,090	+ 9,385	365,752	+ 13,963
1915	100,969	−18,736	387,807	+ 35,963
1916	66,548	−53,157	407,238	+ 55,394
1917	62,378	−57,327	468,312	+116,468
1918	62,421	−57,284	590,455	+238,614
Total	.....	*−117,119*	.....	*+460,347*

Supposing that under normal conditions the observations made for 1913 remained stationary in the subsequent years, the deficit of 1,468,303 female births noticeable from 1913–1918 is surpassed by the relatively slight surplus of 283,228 female deaths, in connection with which it must be noticed that almost two thirds of them (181,330) occurred during the influenza year 1918. In that year 130,585 more females died from influenza and pneumonia than in 1917. After eliminating this extraordinary item, the difference between the number of females deceased in 1918 and in 1913 is decreased to 50,745, and the increase of the number of persons deceased from 1914–1918 would accordingly decrease to 152,643.

The deficit of births observable during the war years, however, produced a deficit of deaths among the infants up to one year of age. From 1914–1918 this deficit amounted to 177,119 for the female sex, so that the total female deaths must appear to be too small by this number. When we try to eliminate this item by considering only the deaths among the *female population of more than one year* of age, we get a surplus of 460,347 deaths among the female population in the years from 1914–1918 together, when we take the death-rate of 1913 as a standard for each of these years. The distribution of the real surplus among the different age classes will be shown by the following investigation based upon the calculation of the death-rates for age groups of five years and for each sex per 1,000 living of the same age.

II. THE COURSE OF INFANT AND CHILD MORTALITY, 1913–1922

Before the war Germany belonged to the countries which had the greatest infant mortality, and it was only during the war and the post-war years that it reached a more favorable place in this regard. The lowest rate of infant mortality reached before the war was that of 1912. In that year only 147 infants per 1,000 live-births died in the first year of life. In the subsequent years this rate was higher only in 1913, 1914, 1915, 1917 and 1918. Within these years the infant mortality rate was greatest in 1914, amounting to 164 per 1,000, owing to the very hot summer of that year. Since the end of the war infant mortality has continued to decrease almost uninterruptedly, which is the more remarkable as neither the increasing birth-rate nor the hot summer of 1921 was able to check this decline. In 1922 infant mortality reached its minimum with a rate of 130, observed for the first time in Germany. The very fact that the expected summer increase was almost completely lacking in the abnormally hot year of 1921 shows that the breast-feeding of infants compared with former times must have made considerable progress. Besides, the scarcity of

foodstuffs prevented the infants from being overfed, which was often the case formerly, thus eliminating one of the principal causes of the high mortality of infants.

Before the war the death-rate of children in the age group 1 to 5 years reached its minimum in 1914, amounting to 13.1 per 1,000 living of the same age. In 1915, when the acute infectious diseases raged in Germany, this rate rose to 16.8 and was afterwards surpassed only by that of the influenza year 1918 (22.0) and by that of 1919 (18.0). From that time the mortality rate of children 1 to 5 years of age has progressively declined, sinking to 11.6 in 1921, thus being considerably lower even than the rate of 1914. As a result of the continuation of the marked decrease in deaths from infectious diseases among children, a further decline of this rate occurred in the year 1922.

The mortality of children from 5 to 10 years of age took an equal course, being subject to the same influences. The first rise, occurring in 1915, was followed after a slight decline by the maximum of 1917 and 1918, and afterwards by a progressive decrease until the minimum was reached in 1921. On the other hand, the epidemics of 1915 did not exercise any influence upon the mortality of children 10 to 15 years of age; nevertheless, mortality in this age group increased uninterruptedly till 1918, afterwards falling more rapidly to a minimum in 1921.

In the various years the course of infant and child mortality for the whole of Germany was as follows:

TABLE III

DEATH-RATES BY AGE GROUPS

Year	Under one year per 1,000 births *			1 to 5 years			5 to 10 years		10 to 15 years	
	Per 1,000 living of the same age and sex									
	Male	Female	Both Sexes	Male	Female	Both Sexes	Male	Female	Male	Female
1913	163	136	151	13.5	12.8	13.2	2.9	3.0	1.9	2.0
1914	*176*	*148*	*164*	13.4	12.7	13.1	3.1	3.0	2.1	2.1
1915	161	135	148	17.2	16.3	16.8	4.2	4.2	2.3	2.5
1916	151	128	140	15.6	15.0	15.3	3.8	3.9	2.5	2.7
1917	161	135	149	15.8	15.2	15.5	4.4	4.3	3.0	3.1
1918	171	143	158	*21.7*	*22.3*	*22.0*	*5.5*	*6.1*	*4.1*	*4.8*
1919	157	130	145	18.3	17.7	18.0	3.8	3.9	2.6	3.0
1920	144	118	131	14.3	13.4	13.9	3.2	3.2	2.3	2.3
1921	146	120	134	12.2	11.1	11.6	2.6	2.4	1.9	1.8
1922	...	...	130	11.1	10.2	10.7	2.1	2.0	1.7	1.7
1923	...	...	132	....	....	....	...	...	...	...

* For the years 1915–19: thousandfold value of probabilities of life.

III. MORTALITY AT AGES 15–20 YEARS

The difference between the death-rate in the male and the female sex begins to be noticeable in the age group 15 to 20 years, at which age the death-rate, after its physiological minimum reached between 10 and 15 years, slowly commences to increase again. During the war the effect of sex on mortality within this age group rose to an extraordinary degree, as in this age group war losses were already felt. The natural course of mortality of this age group is, therefore, revealed by the mortality of the female sex alone. The death-rate of females began to rise as early as 1915, and in 1918 was almost three times as great as that of 1913 and 1914. In the latter years the death-rate was only 3.1 per 1,000 living of the same age and sex, while in 1918 the death-rate amounted to 8.9, owing to the epidemic of influenza raging at that time from which persons of this age group suffered most of all. Though the death-rate of the female sex since the end of the war has decreased almost as rapidly and markedly as it had risen, the minimum of 1913 and 1914 was not attained till 1921. The mortality of the males took the same course. It rose from 3.4 in 1913 to 24.2 in 1918; and, being 4.0 in 1921, it was considerably greater even in that year than in 1913. During the war the survivors of this age group suffered from shortage of foodstuffs more than the other age groups, as the war years were for them the period of physiological development. Owing to this circumstance, the increase of mortality from tuberculosis during the war is particularly great in this age group.

The death-rate in the 15–20 year age group is shown in the following table.

TABLE IV

DEATH-RATE IN THE 15–20 AGE GROUP

Year	Deaths per 1,000 living of the same age and sex		Year	Deaths per 1,000 living of the same age and sex	
	Male	Female		Male	Female
1913	3.4	3.1	1918	*24.2*	*8.9*
1914	7.0	3.1	1919	6.4	5.1
1915	11.5	3.4	1920	5.3	4.1
1916	11.4	3.7	1921	4.0	3.2
1917	18.3	4.8			

To estimate the importance of these figures, they may be compared with the corresponding rates in the Netherlands—the mortality of the Netherlands being the lowest of all the countries of Europe at present as it was before the outbreak of the war. There the average death-rate of the age group in question amounted to 3.0 in 1909–10 for both the sexes, and the average death-rate in 1920–21 amounted to 2.9.

A much larger decrease, however, is shown by the death-rate in the younger age groups in 1921, the most favorable year of all, when it is considered by itself. For in this year the death-rate was 2.5 for the male sex and 2.6 for the female sex of this age group. These figures show that the German rates of 1913 were already competing with those of the Netherlands, while there is still a considerable difference between the rates of the two countries for 1921.

IV. MORTALITY IN THE GROUP 20 TO 45 YEARS OF AGE

The Effects of War upon Mortality

At the ages of 20 to 45 years mortality increases from year to year. This increase, however, is so slight that the curve of mortality as compared with the other age groups forms almost a horizontal line. When this group is subdivided according to sex the results are somewhat different, however, because the mortality curve of the female sex is higher than that of the male sex for the 25–35 age group. The causes of this phenomenon, the effects of pregnancy and childbirth, and the resulting increase of the tuberculosis mortality rate among females, are well-known facts among medical experts. As will be shown, these proportions were, of course, completely transformed by the war.

The mortality of the male sex increased in the different groups of the five years of duration of military service in proportion to the extent of active participation in the war. The group of 20–25 years of age is therefore characterized by the highest mortality rate throughout the war. While in 1913 the death-rate of this group numbered only 4.4 per 1,000 living of the same age and sex, it rose to 66.9 in 1915, the latter rate being the maximum attained by this age group. In the two subsequent years this enormous rate declined, but rose anew to 58.8 in the influenza year of 1918. Even during the post-war years the death-rates were far higher than the standard rate of 1913, because this age group took the most active part in the political troubles following the revolution. As to the female sex, the death-rate of this age group did not really rise until 1917, and during the post-war years decreased with a striking slowness—the influenza epidemics of 1919 and 1920 and the large increase of the number of puerperal septicemia cases (resulting from abortions) exercising a retarding influence.

The age group of 25–30 years of age shows much lighter war losses than in the 20–25 group. The maximum death-rate of this group, which was reached in 1915, amounted to only 45.7. But the mortality of the male sex of this age group was smaller than that of the preceding age group even during the post-war years. This fact, which suggests abnormal conditions, is the result of the higher mortality in

the younger class resulting from violent deaths. As for the female sex, mortality as in the preceding age group did not begin to increase before 1917. As in other countries, the group of 25–30 years of age suffered most from the influenza epidemic of 1918, reaching in that year the highest death-rate of all the groups considered in this section (12.7). For reasons similar to those given in the case of the preceding age groups, this category did not approximate the pre-war rate before 1921.

In the group of 30–35 years of age, mortality took a similar course, excepting for the fact that the death-rate of the male sex for 1915 was not higher than that of 1918. After the war, the decrease of the mortality among males was so great that in 1921 the death-rate of males was even lower than its pre-war rate. In this way the death-rate of males sank below the rate for females, as is regularly the case for this age group in normal times; the difference between the death-rates of the two sexes in favor of the male sex, however, increased. The decline of the mortality of the female sex of this age group in the post-war years was likewise slow in coming; the death-rate of 1919 and 1920 exceeded even that of 1917, like the death-rate of the two preceding classes, and even in 1921 did not quite fall to the rate of 1913.

In the 35–40 age group, the course of mortality among males during the war years presented a different aspect. War losses began to be less important, so that the year 1918 showed the highest death-rate (25.0), as compared with 6.4 in 1913. After the war, a remarkable change took place as in this age group, too, the mortality of the male sex was smaller than that of the female sex due to the greater decline of mortality from tuberculosis among the former. The death-rate of females, which during the war increased a relatively slight degree, was in 1921 again as high as in 1913.

TABLE V

DEATH-RATES FOR GERMANY

(Per 1,000 Living of the Same Age and Sex*)

By 5-year age periods for ages 20–45

Year	20–25 yrs.		25–30 yrs.		30–35 yrs.		35–40 yrs.		40–45 yrs.	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
1913.....	4.4	4.0	4.6	4.7	5.1	5.3	6.4	6.1	8.6	6.9
1914.....	37.8	4.1	32.5	4.9	20.3	5.4	12.9	6.2	10.0	7.1
1915.....	*66.9*	4.1	*45.7*	4.7	*32.9*	5.4	20.4	6.3	12.7	7.2
1916.....	52.9	4.4	32.9	5.0	25.6	5.7	20.8	6.6	12.5	7.5
1917.....	44.1	5.4	27.8	5.9	22.7	6.6	19.5	7.7	16.4	8.9
1918.....	58.8	*11.6*	40.4	*12.7*	*33.0*	*12.4*	*25.0*	*11.7*	*18.3*	*11.5*
1919.....	8.1	6.2	7.7	6.7	7.3	7.2	7.5	7.6	8.5	8.3
1920.....	7.1	5.8	6.7	6.8	6.3	7.0	6.5	7.2	7.4	7.6
1921.....	5.9	4.4	5.3	5.1	5.0	5.5	5.6	6.1	7.0	6.7

* Population of the German territory according to its actual size in the different years estimated according to age and sex.

As to the category of 40–45 years of age, the death-rate of males did not begin to rise rapidly before 1917. In 1918 it exceeded this rate only slightly. For this reason, it bore some resemblance to the course of the female death-rate. After the war, the two death-rates were actually identical, while before the war there was always an excess in mortality on the side of males. This excess set in again in 1921, though only to a moderate degree. Compared with 1913, the difference of the male death-rate for 1921 was far greater than that of the female death-rate, so that the former difference between the death-rates of the two sexes had nearly disappeared.

V. MORTALITY AT AGES FROM 45–70 YEARS

The Selective Effects of Famine

The age groups of 45–70 years are distinguished by a rapid increase of the difference between the death-rates of the two sexes, the death-rate of the male sex rising more rapidly than that of the female sex. The difference between the two sexes as to certain causes of death in this age group suggests the explanation of this abnormal phenomenon, for the sharp rise of the male death-rate is produced particularly by tuberculosis, violent deaths and diseases of the circulatory, respiratory, nervous, urinal, and genital system. These items, commonly acknowledged before the outbreak of the war, kept their value also in the post-war years, but the difference between the death-rates of the two sexes is far less marked, due to the fact that the male death-rate declined more than that of the female sex, and was for each group of five years lower than its pre-war minimum. This result is a phenomenon of high social importance, and may, therefore, be made the object of special investigation.

The age group of 45–50 years suffered but little from the war and the influenza epidemic raging in 1918. So the death-rate of males from 1916 to 1918 was lower than that of the younger classes down to the group 15–20 years of age. In 1919 the death-rate of the males of 45–50 years of age (10.8) had already sunk below the death-rate of 1913 (11.6), and decreased to 9.0 in 1921. The death-rate of the female sex, however, did not reach the death-rate of 1913 (8.5) till 1921, numbering 8.3 in the latter year. The difference between the death-rates of the two sexes consequently was 3.1 in 1913, and only 0.7 in 1921.

The mortality of the age group of 50–55 years took the same course. The male death-rate, which did not begin to rise before 1917, fell rapidly after the war. By 1919 it sank below the last pre-war rate (16.6), numbering 15.4 in this year. In 1921 it fell as low as 13.0.

This rise of the female death-rate, on the contrary, began one year earlier, *i. e.*, in 1916, while the rate of 1913 (11.8) was not reached until 1921 (11.4). The difference between the death-rates of the two sexes, amounting to 4.8 in 1913 and 1.6 in 1921, has thus diminished, as in the preceding age group, in favor of the male sex.

The category of 55–60 years of age is of particular statistical importance, because at this period death-rates greater than the average begin to appear. This phenomenon is met with even in countries which have very different total rates. Only the category of under one year and in some countries the group of one to five years also have death-rates over the average. The category of 55–60 years of age is the first that, during the years of war, reached its maximum death-rate in 1917 instead of in 1918, as far as the male sex is concerned. Anticipating here, it might be mentioned that this was the case for all of the following age groups. In the case of the female sex, however, it was the category of 75–80 years of age which was the first to reach its maximum death-rate in 1917.

This fact shows that the effects of the famine rather than of the influenza epidemic of 1918 caused in this year the marked increase of the death-rate of the older age groups. It is a well-known fact that the advanced age classes suffered far less from that epidemic than the younger ones. The prior maximum rise of the death-rates of males at ages of more than 55 years reveals that it was the male population which succumbed first to the selective effects of the famine. This phenomenon is, on the whole, in accordance with what Suer [1] observed, that is, that during the famine in Paris from September, 1870, till February, 1871, the number of the deaths among the male population of 40–60 years of age surpassed the corresponding average figures of the two preceding years of peace by 2.49 times, and the number of the deaths among the female population of the same age surpassed them by 2.27 times; while the number of the deaths among the male and the female population at ages of more than 60 years surpassed them in the same degree, by 2.67 and 2.71 times respectively. It is true, Suer's computations are too crude and his age groups too wide to be equivalent to the refined German mortality statistics which do not deal with crude values at all. To be sure, the very severe winter and the want of coal which began at that time may have exerted an influence more deleterious for the male than for the female sex, though these two conditions were also prevailing during the famine at Paris.

From the selective effects of the famine, which began to increase more and more in 1917, the principal sufferers were tuberculosis

[1] Suer H.: *Étude sur la mortalité à Paris pendant le siège.* Paris, 1872, p. 76.

patients, the insane, and decrepit people, though the latter play a part only among the higher age groups. As for tuberculosis patients, these suffered most in the age groups of 15–30, and 50–70 years, and in the latter category males suffered more than females. The larger number of deaths among the insane may also have occurred in the latter age period of 50–70 years—the male sex among the insane also showing a higher mortality than the female sex. The German hospitals for the insane were compelled to practise race hygiene during the years of famine, with the result that in 1917 the fifth part (20.1 per cent) of the male and the sixth part (16.5 per cent) of the female inmates died, while in 1911 to 1913 the average death-rate was only 5.7 and 5.6 per cent respectively. The differences between the male and the female sex regarding deaths from tuberculosis and insanity reveal the cause both of the earlier rise of the death-rate of the male population of 55–60 years of age and its more marked decline after the war; for above 50 years of age the male sex was subjected to a stronger selection than the female sex of the same age. The male death-rate at the age of 55–60 years sank to 19.6 in 1921, thus being by 4.5 lower than the rate of 1913, while this difference, in the case of the female sex, only amounted to 0.8.

In the category of 60–65 years of age the death-rate of the male sex in 1917 shows a rise still more rapid than that of the preceding category, as from that age group on the selection among the decrepit people also began to play a part. Consequently, in 1917, the difference between the male and the female death-rate rose to one fourth, while in 1913 the female rate was lower than the male death-rate by one seventh. Afterwards, the difference diminished more rapidly and intensely, the male death-rate sinking to 30.2, and the female rate to 26.4 in 1921, while the corresponding figures of 1913 were 35.8 and 27.9 respectively.

In the category of 65–70 years of age the rise of the female death-rate in 1918 was far less marked, because in 1917 this rate had increased more strongly than in the previous years. The effects of selection became more and more obvious among the female sex, too. The final result of these selective effects was that even in this age period the death-rates of 1921 for each sex were lower than those of 1913: the death-rate of the male sex, amounting to 52.8 in 1913, sank to 47.2; the rate of the female sex, which was 44.5 in 1913, sank to 42.0 in 1921. The famine of 1917 and 1918 had radically thinned the ranks of the decrepit persons of this age group.

TABLE VI

DEATH-RATES FOR GERMANY

(Per 1,000 Living of the Same Age and Sex)

By 5-year age periods for ages 45–70

Year	45–50 yrs.		50–55 yrs.		55–60 yrs.		60–65 yrs.		65–70 yrs.	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
1913	11.6	8.5	16.6	11.8	24.1	17.4	35.8	27.9	52.8	44.5
1914	12.1	8.6	17.3	12.0	24.1	17.4	36.6	28.2	54.8	45.7
1915	12.8	8.8	16.6	11.9	24.2	17.4	36.0	27.7	54.5	45.1
1916	12.4	9.3	16.7	12.4	24.3	18.1	36.6	28.7	56.5	47.5
1917	15.9	10.7	20.3	14.5	*29.3*	21.3	*43.8*	33.0	*68.5*	54.6
1918	*16.9*	*13.3*	*21.2*	*17.7*	29.1	*23.6*	42.9	*35.4*	67.4	*57.9*
1919	10.8	19.9	15.4	13.5	22.5	19.5	34.2	29.9	54.4	49.4
1920	9.6	9.1	13.9	12.5	20.6	18.0	32.0	27.7	50.8	45.9
1921	9.0	8.3	13.0	11.4	19.6	16.6	30.0	26.4	47.2	42.0

VI. MORTALITY AT AGES OF MORE THAN 70 YEARS

The Selective Effects of Famine upon Older Ages

The number of people who are fortunate enough to arrive at an age of 70 years is very limited. According to the last German life-table for 1910–11, which shows the course of mortality among that generation, 30 per 100 living-born males reached the age of 70, and 36 per 100 living-born of the female sex, *i. e.*, two thirds of that generation died at younger ages. The survivors' chances, also, of arriving at a still higher age, are small; for the average expectation of life at the age of 70 years amounted to 7.90 for the male and 8.35 for the female sex at that time. And the ranks of those whose durations of life surpass this average number of years are thinned very soon; for the age of 80 years was reached by only 10 and 13 persons per 100 living-born of the male and female sex respectively. With regard to the age of 90 years, this percentage sank below 1 for the male, and to 1.1 for the female sex.

This natural course of mortality was disturbed by the war and its privations in a most striking way. That part of the population most innocent of the war suffered most from it. The effects of famine naturally affected the oldest people first and most intensely, as their worn-out bodies were least able to resist the changes of nutrition. The pitiful dramas which occurred in the lives of these people cannot be revealed by barren figures; but the language which the latter speaks is more impressive for those who understand how to interpret it and form their opinions only upon indisputable data.

While in all the younger age groups the uninterrupted increase in mortality did not begin before 1916 or 1917, it set in among the older age groups as early as 1914, affecting the male sex first. In the category of 70–75 years of age the death-rate of the male sex rose unin-

terruptedly from 80.2 in 1913 to 106.0 in 1917, while the death-rate of the female sex in 1915 was somewhat lower than that of 1914. By 1917 the female death-rate increased to 90.8 as compared with 78.8 in 1916, and rose to 95.2 in 1918, while the male rate began already slowly to decrease in that year. Consequently, in 1918 the two death-rates were nearest to each other. During the post-war years the decline of the male death-rate was somewhat more marked than that of the female rate, which produced a further diminution of the difference between the two rates. In 1921, both the sexes had lower death-rates than in 1913, the most worn-out people having died by this time.

In the category of 75–80 years of age the power of resistance of the female sex was likewise broken by 1917; for in this age group the highest death-rate of the female sex is already met with in 1917, as is the case for the male sex also in the category of more than 55–60 years of age. Thus, the stronger power of resistance of the weaker sex held true even amidst the greatest privations, nor did it begin to waver until 1916—two years later than was the case with the males of this age group. In 1917, the maximum death-rate of males numbered 167.0, that of the female sex 150.3. Both these maxima were higher by one third than the corresponding rates of 1913. By 1918, after the decease of the weakest people the decline in mortality began, favored by the meteorological circumstances which were much more advantageous in that year than in 1917. In 1921 both the male and female death-rates were lower than the respective rates of 1913, the difference, however, being but a slight one.

In the category of 80–85 years of age an excess in the mortality of the female sex at later ages is met with for the first time, though only in one year, 1919. In this age group the increase of mortality began in 1914 for females, too, and rose rapidly until 1917, as was also the case for males. During the latter year, more than one quarter of the male population and nearly one quarter of the female population of this age period died, while in 1913 only one sixth of each sex died. The male death-rate was lower than the death-rate of the female sex in one year, that is in 1919, and sank below the level of 1913 as a result of the fact that the male death-rate in the years 1918–1921 decreased more than the female death-rate.

In the age period 85–90 years the difference between the death-rates of the two sexes as well as the difference as to the rise of the death-rates of the two sexes diminished more and more. In 1918 and 1919, the death-rate of males was lower than the female rate, after the latter (398) had nearly equalled the former (406) in 1917. The rapid rise of the death-rates for both sexes had set in by 1914, so

that the curves of both the rates from 1913 to 1917 form almost a straight line. Two fifths of the male and also of the female population in this age group died in 1917, so that the number of the living people of this age group naturally was greatly reduced in the subsequent years, the more so as a selected group greatly reduced in number entered this age group. This number of living people of this category in Germany sank from 27,057 in 1915 to 23,367 at the beginning of 1918 for males, and during the same time from 40,625 to 34,822 for females. The death-rate of the selected survivors in 1921, however, was scarcely lower than that of 1913.

TABLE VII

DEATH-RATES FOR GERMANY

(PER 1,000 LIVING OF THE SAME AGE AND SEX)

By 5-year age periods for ages 70–90

Year	70–75 yrs.		75–80 yrs.		80–85 yrs.		85–90 yrs.		Over 90 yrs.	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
1913	80.2	72.0	123.9	113.1	189	176	274	258	387	373
1914	82.8	73.5	129.6	119.6	194	183	299	285	426	397
1915	84.2	73.3	129.7	118.4	200	184	316	286	450	406
1916	88.1	78.8	139.5	128.5	214	197	332	321	493	461
1917	*106.0*	90.8	*167.0*	*150.3*	*263*	*238*	*406*	*398*	*578*	*559*
1918	101.5	*95.2*	151.7	149.9	235	234	352	363	533	542
1919	85.6	80.8	135.1	131.6	208	209	288	289	415	414
1920	79.6	75.9	126.5	121.5	196	191	285	267	376	395
1921	75.9	70.2	121.2	112.8	186	178	270	255	355	321

(Upon the statistical records in vol. 276 of the *Statistik des Deutschen Reiches* and in the periodical *Wirtschaft u. Statistik*, 1923, p. 455.)

With the maximum death-rate of the oldest age period of over 90 years of age, which in Germany in 1919 did not comprise more than 11,318 people, among whom there were 86 persons of more than 100 years of age, the tragedy of war and famine comes to its end. The average number of years which persons of 90 years of age are likely still to pass through is somewhat more than two years. This short space was reduced to a minimum by the war. With an alarming rapidity, the death-rate of the male sex, which numbered 387 in 1914, rose to 578 in 1917; that of the female sex rose from 373 to 559. Thus of all the men and women of this category, more than one half died during that year. Owing to the greater selection among the male sex, the death-rate of the males sank below the female rate in the three subsequent years. Among the female sex the strong selection continued even in 1918, while afterwards its death-rate also decreased rapidly. Due to their tested power of resistance, the few survivors got more favorable chances as to the further duration of their lives in 1921 than even in 1913.

Thus the fact that the German death-rate soon after the war decreased to a minimum never attained before is the result of the effects of selection which the German population was subjected to during the years of the worst privations. The effects of this selection would, of course, become much more obvious if the death-rates enumerated in this article could be separated according to rural and urban population. But the mere fact that the death-rate of the urban population after its greater increase during the war years sank again below that of the male population by 1921, and has continued to lower minimums, shows that the effects of selection on the urban population must have been much more intense and will therefore continue much longer than among the rural population.

OUR UNSTABLE DOLLAR AND THE SO-CALLED BUSINESS CYCLE

By Irving Fisher

MAIN RESULTS

This article gives the final results—final, at least, for the present—of a work begun two years ago, of which reports of progress have been made from time to time.[1]

In the two years during which the subject has been intensively studied, a number of different methods and data have been tried in an endeavor to obtain the maximum correlation between the fluctuations of the dollar and the volume of business. By the method and data set forth in this Journal, December, 1923, a correlation was found of +.79, or, popularly, 79 per cent. By those described in the Pollak Foundation volume, 86 per cent was found. In this article 94.1 per cent ($+.941 \pm .008$) is reached.

Seldom before has a correlation so high been found in the efforts to explain "the business cycle." It seems certain, humanly speaking, that wherever the dollar—our yardstick of commerce—suffers wide fluctuations in purchasing power, those fluctuations largely predetermine or, at any rate, precede closely related fluctuations in trade.[2]

THEORETICAL BACKGROUND

It has long been recognized that a rising price level temporarily stimulates trade and that a falling price level depresses trade. Otherwise expressed, monetary depreciation stimulates, and monetary appreciation depresses, trade. For society as a whole, and in the long run, both the appreciation and the depreciation are, of course, evils. The stimulation to business from rising prices, for instance, does not mean a benefit to all classes. It means an abnormal gain of the profit-

[1] Namely, at the dinner of the American Statistical Association in New York City, December 15, 1922; at the meeting of the National Monetary Association, in New York City, June 7, 1923; in "The Business Cycle Largely a 'Dance of the Dollar'," *Publication of the American Statistical Association*, December, 1923; at the meeting of the American Statistical Association, Washington, D. C., December, 1923; in the volume containing the addresses at the last named meeting, published under the title "The Problem of Business Forecasting" by the Pollak Foundation for Economic Research, 1924; at the dinner of the American Statistical Association in New York City, November 7, 1924.

[2] It should be clearly understood, however, that both the purchasing power of the dollar and the volume of trade, so far as this investigation goes, are restricted to the special meanings explained below. The "purchasing power" here considered is that over commodities *at wholesale*, and the "trade" is that *in basic materials*. The relationships here found do not apply completely to the more general meanings of these two terms applicable to the Equation of Exchange as worked out by me several years ago and later, more accurately, by Carl Snyder.

taker, or "profiteer," at the expense of his creditors and employees; and in the end even the profit-taker is usually injured.[1]

These relationships have been recognized chiefly on *a priori* grounds. The reasoning is simple and obvious: When producers get higher prices they do not at first have to pay correspondingly higher wages and salaries; much less do the rent and interest which they pay rise. This lagging behind of interest, rent, salaries and wages—not to mention raw materials which lag for a somewhat different reason—is inevitable because the laggards are fixed by contracts or understandings. Evidently the lagging of these important elements of expense practically involves a lagging of total expenses behind total receipts. Consequently profits, the excess of receipts over expenses, tend to be increased. Reversely, falling prices hurt profits.

Since the profit-taker is the captain of industry, on whose decision depends the rate of output, it is further inevitable that when profits increase, industry is expanded and business booms; while, when profits decrease, industry is contracted and business is depressed.

The present paper is little more than an inductive verification of these established truths in their application to "the business cycle"; and yet I venture to think that the conclusions which I have reached will seem novel because "the business cycle" has not generally been mentally associated, as it should be, with inflation and deflation.

In a general way the thesis that inflation stimulates, while deflation depresses business has often been inductively verified where attention has been fixed on either inflation or deflation alone—because they are made conspicuous by being long continued or very rapid. The phenomenon was, for instance, observed as to the long period of deflation after the Civil War culminating in 1896; and again as to the long period of inflation beginning in 1896 and culminating (in England and the United States) in 1920; and again as to the recent rapid inflation in Germany; and again as to the drastic deflation of 1920–22 in the United States, England and Czechoslovakia.

But, strangely enough, such observations have been conspicuous by their absence when the subject matter under consideration is "the business cycle," implying an *alternation* of booms and depressions; and this in spite of the fact that, in the last few years, many statistics have been accumulated in the effort to explain such alternation. We now have nearly fourscore forecasting agencies to help the business man, none of which bases any part of its predictions on the idea that alternate booms and depressions are due to alternate inflation and deflation. In fact some of the best of these forecasting agencies

[1] See, *e. g.*, Irving Fisher, *Stabilizing the Dollar*, Macmillan, 1920, pp. 76–77.

omitted the price level curve altogether through the disturbed war period—the very period when the price level factor was the most important and when, as we shall see, it could be used to explain most fully the ups and downs of business. Again, some of the agencies have tried to "eliminate the secular trend" from the price level, a procedure almost useless not only because the price level cannot properly be said to have any secular trend, or long time tendency in one direction, but also because, to eliminate any such supposed trend is to throw away a part of the rise or fall, which, as we shall see, is so vital a factor.

The chief reason why the rôle of inflation and deflation was missed by the forecasters is that they missed the clear distinction between high prices and rising prices and, likewise, between low prices and falling prices. In short, they scrutinized the price level but not its *rate of change*.[1] Finding no alternation in the curve representing the price level they did not see how that curve could be used to explain the alternation in business. Had they scrutinized and plotted the *rate of change* of the price level they would have noticed an alternation and also noticed a correspondence between that alternation and the alternation in business. The task set for this study is to discover how great that correspondence is.

METHODS USED

For this purpose the materials used for the statistical calculations are chiefly two: "P", the index number of wholesale prices of the United States Bureau of Labor Statistics, and "T", the Index of Trade of Professor Warren M. Persons.[2] The period covered by the trade series is from August, 1915, to March, 1923,[3] inclusive.

I take this opportunity to advocate the use of an index of the *physical volume* of trade in place of the vague and almost meaningless index of

[1] The idea of the *rate of change* of the price level, or of its reciprocal, the purchasing power of the dollar, was emphasized in my *Appreciation and Interest*, August, 1896 (Publications of the American Economic Association, Vol. XI, No. 4). It was used by Prof. J. M. Clark (*Journal of Political Economy*, March, 1917, Vol. XXV, No. 3).

The same idea of a rate of change or derivative, and the inverse idea of cumulation (or "quadrature" or integral), have been applied with success in somewhat different fields by Charles N. Edge and by Karl G. Karsten ("The Theory of Quadrature in Economics," *Journal of the American Statistical Association, March, 1924*).

[2] Corrected for secular trend and seasonal variation and published in the (Harvard) *Review of Economic Statistics*, April, 1923, p. 71 ff. This index covers only basic commodities such as pig iron production, cotton consumption, employment and freight.

[3] This period seemed the most suitable for the purpose (namely to obtain the best estimate of the true influence of price changes on business) chiefly because, during this period, the price changes were so great. 1914 was excluded because the previous studies made showed a definite influence of the sudden outbreak of war, an influence irrelevant in this study. The period ends at February, 1923, simply because that was the latest date for which data were available when the methods here employed were first applied. The period chosen, as well as the trade index, differ slightly from those employed in the previous two studies. The price series goes back of 1915 to February, 1906, as required by the "lags" explained below.

"business" so commonly employed hitherto. "Business" is a cloudy concept. An index of "business" consisting of a mixture of three such essentially different categories as quantities, prices, and values, seems to me as absurd as an index of "weather" which would jumble together, in a single average, such unlike elements as temperature, humidity, cloudiness, barometric pressure, and wind velocity. No meteorologist would insult his science by such an average index of the weather. The atmospheric weather is not to be put in any one figure. No more is the economic weather. But physical volume of trade has a definite and important meaning or set of meanings, just as does temperature or barometric pressure.

The trade curve is slightly smoothed [1] in order to rid it somewhat of minor accidental irregularities. Thus the two curves expressing our statistical raw materials from which all other calculations flow are: the "P", or price index, curve (shown in Chart 1), and the "T", or trade, curve (shown in Chart 4), P and T being taken in the special sense explained above.

At first glance these two curves reveal no evident relationship.[2] But this is chiefly because their relationship is not directly between the heights (ordinates) of these curves but between the *slope* of the P curve (*i. e.*, the rate of change of the price level) and the *height* of the T curve.

To bring out this relationship more clearly we employ a derivative of P, namely, the curve P' (also shown in Chart 1) such that the *height* of P' is expressed by the same figure as the *slope* [3] of P. Thus, for April, 1917, when P is *steepest upward*, P' is *highest;* while at November, 1920, when P is *steepest downward*, P' is *lowest.*

In the former instance (April, 1917) P is ascending at the rate of 72.6 per cent per year so that the height of P' in that month is also 72.6 while, in the latter instance (November, 1920), P is plunging downward at the rate of 97.8 per cent per year so that P' is 97.8 points below the zero line.

Evidently: P' is on the zero line whenever P has a zero slope, *i. e.*, is horizontal; P' is above zero when P is ascending; and P' is below zero when P is descending.

With P' thus available in place of P, the eye merely has to compare two *heights,* those of P' and T. There is now no difficulty in seeing a

[1] By means of a three months' moving average, the central month in each case being weighted twice as heavily as the month on either side.

[2] It is interesting to observe that the correlation between P and T is 54 per cent (or, by first differences, is 51 per cent) for the period 1915–23.

[3] The slope for any given month is measured by subtracting the index number for the *preceding* month from that for the *succeeding* month and reducing the result to a percentage of the given or intervening month. This percentage, being for two months, is multiplied by six to give a *per annum* rate.

resemblance. The curve T evidently copies P' closely, T following after P' with a few months' "lag" to be discussed later.

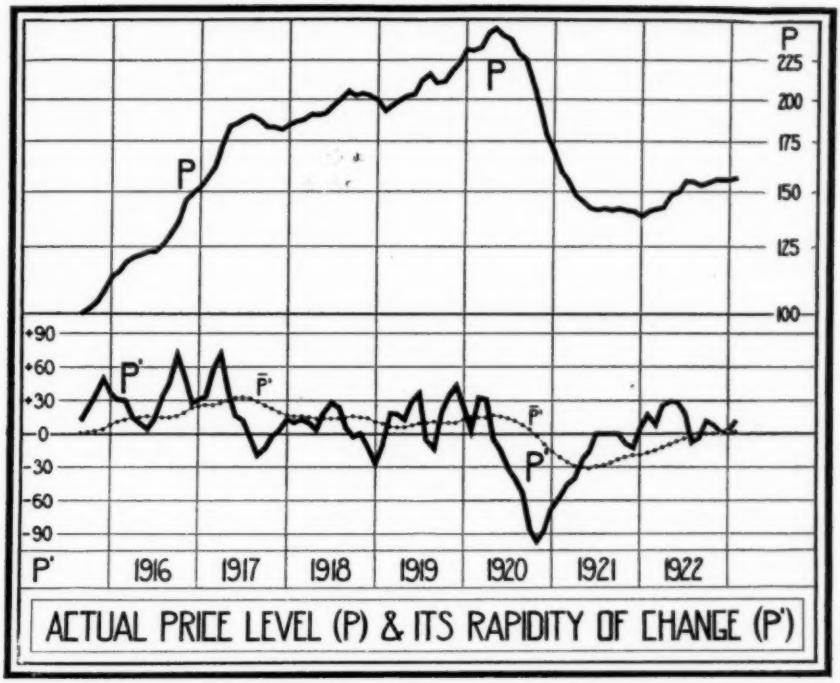


CHART 1

Showing how the slope of the upper curve P (price level) is represented by the height of the lower curve P'. (As to $\overline{P}'$ see Chart 4.)

We note at once that P' supplies an oscillating barometer without requiring any of the corrections for secular trend and seasonal variation found necessary in most "cycle" data, including the "T" here used.

The correlation between P' and T, with a lag of seven months, is 72.7 per cent. This is the maximum correlation; that is, it is greater than that (71.9 per cent) for six months or that (71.5 per cent) for eight months or any other length of lag. This is a high degree of correlation. But it can be greatly bettered—increased, in fact, to 94.1—simply by substituting for this *fixed* lag of seven months a *distributed* lag spread over one, two, three, etc., months according to the principles of probability.

So far as I know, this is the first attempt to distribute a statistical lag. The principle involved would seem to be a general and useful one. It ought to be possible in many other studies to find "the law of the

lag" as described below. The reason for distributing the lag is that the full effect of each P' item is extremely unlikely to be felt at only one instant, such as seven months later, and not felt at any other time either earlier or later than this seven months. Can we imagine that the sharp rise of prices in April, 1917, did not begin to affect trade until November, 1917, and that then the whole effect suddenly exploded, as it were? It is far more probable that the influence began at once, showing itself in the very next month, May, and that it then gradually increased to a maximum a few months later and thereafter tapered off indefinitely according to the probability distribution. It stands to reason that some industries respond more promptly than others and that even the same industry feels the influence not as a sudden jolt but in a graduated crescendo and diminuendo.

But what is the exact nature of this probability distribution? There are many types of probability curves. It would seem that we should select, as the most appropriate to our present problem, that type which most nearly accounts for the behavior of T.

The type of distribution finally selected is shown in Chart 2, which, in particular, represents the distribution of influence of that price-change, or P', which occurred at April, 1917 ("A" on Chart 2)—a spurt upward of the price level.

To visualize this distribution of influence—beginning in May, then increasing and afterwards diminishing—we may think in terms of the analogy of a gunner shooting at a target. According to this analogy we conceive of the horizontal line drawn to the right of April as representing not only time, but a physical road. Our imaginary gunner shoots forward on this time road, so to speak, a number of bullets proportional to the April P'. Since April's P' was 72.6 (*i. e.*, prices were then rising at the rate of 72.6 per cent per annum), we suppose our gunner to shoot 726 bullets from a vertical pile (the hollow circles) extending upward beyond the limit of the diagram. This column may be regarded merely as the ordinate, P', for April, 1917, with the scale in Chart 1 greatly magnified. The circles are hollow to signify that the bullets are no longer there, but have been shot ahead.

The effects of P' are thus shot forward and redistributed over the road in such a manner that if, at each milestone (or month) those falling there are re-stacked in a column, the resultant series of columns month by month (their tops being connected by a curved line) will form approximately the probability curve shown in Chart 2.

In short, Chart 2 pictures the distribution of the lag between the cause P' and its effects. This distribution curve is the one which, among the many tried, best fits the facts.

Besides the types which were tried, were others which were rejected without trial simply because, *a priori*, they did not look promising. The labor involved in an exhaustive examination is prohibitive; for, under each type of distribution, it is necessary to try numerous sub-varieties.

It is therefore quite possible that the type finally chosen is not absolutely the best. But we may be sure that it is not far from it, if for

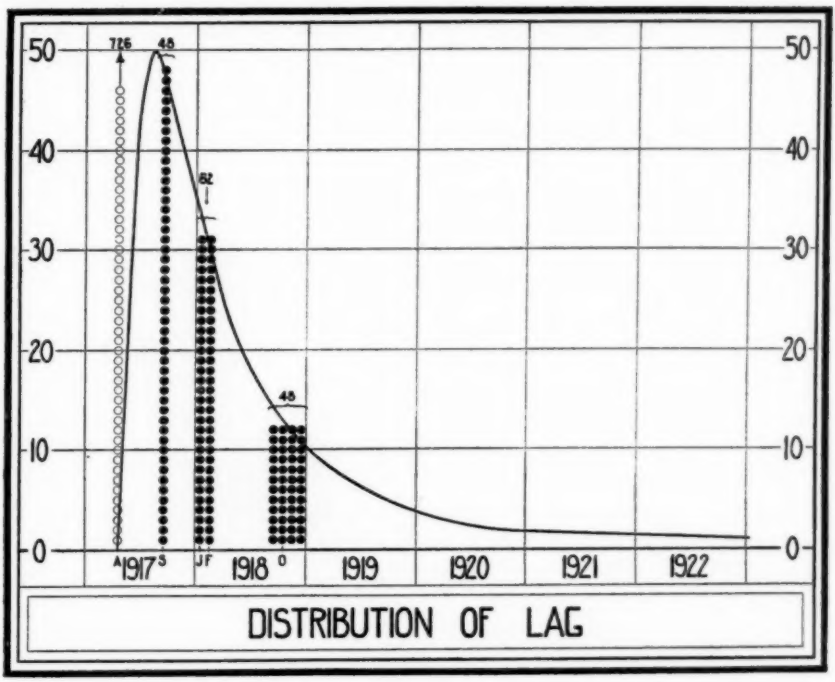


CHART 2

Showing how the pile of 726 bullets representing P' at April 1917 is distributed over the months following. The letters A, S, J, F, O stand for April, September, January, February and October.

no other reason than simply because there remains so little *room* for improvement, between the extraordinarily high correlation which this method yields and 100 per cent.

The Chart shows that, while the influence of this April rise in prices began immediately, the influence was not very great in the first month, May, 1917—only 3 per cent of the total influence (bullets) being then felt. A greater degree of influence (6 per cent of the total) was felt in June, 7 per cent in July, and 7 per cent in August. After this point the intensity gradually diminishes. A little less than 7 per cent was

felt in September (the "*S*" of Chart 2) as pictured definitely by the pile of 48 bullets. In January ("*J*") and February ("*F*"), 1918, we find 4½ per cent and 4 per cent shown by two piles, or 8½ per cent altogether; while later, where the four piles are situated it requires four months (September, October ("*O*"), November and December of 1918) to make up together scarcely 7 per cent, or 48 bullets, the same as for the one month of September, 1917.

Evidently the curve of Chart 2 is asymmetrical about its mode at 3½ months.

Yet, in a certain geometric or logarithmic sense, the distribution is symmetrical about 9½ months as a mode.

It was this latent symmetry, rather than empiricism, which first recommended this type of distribution. The ordinary (arithmetically) "normal" distribution would involve the apparent absurdity of bringing some of the effects earlier than their supposed cause.*

We can best see the symmetry latent in Chart 2, by mentally laying off on the *X*-axis successive intervals of time increasing successively in length according to *geometric* progression. For instance, instead of dividing the horizontal distance into successive months, which are equal in length, let us conceive it divided into successive intervals, each constituting an addition, say, of one per cent of the distance from the origin, or any other constant *percentage*.

In such a series of time-subdivisions, a 25 per cent increment, or interval, comprising (approximately) the ninth and tenth months (January and February, 1918) will be found to contain more bullets (*i. e.*, a greater fraction of the total effect) than will any other 25 per cent interval.

In a geometric sense, then, the mode is not at 3½ months, but at about 9½ months.

It will further be found, that on either side of this mode or maximum, so conceived, the distribution over the successive 25 per cent intervals is symmetrical. Thus, in the fifth month (which constitutes one of the 25 per cent intervals), approximately half as far from the origin as the mode, we find the same number of bullets as in the four months (also constituting a 25 per cent interval), at about *twice* the modal distance.

Similarly the effects at 2/3 and 3/2 of the modal distance are equal, as in general are those at m/n and n/m of that distance.[1]

[1] To make this inherent symmetry apparent to the eye it is only necessary to transform, or distort, Chart 2 into Chart 3. On this Chart, "*S*" stands for September, "*J*" for January, "*F*" for February, and "*O*" for October, as in Chart 2.

This distribution preserves all areas but so modifies the widths of the 25 per cent intervals as to make them *equal*, while modifying the heights in the inverse ratio. Thus the area of 9–10 months is kept unchanged, and that of 17–20 months is compressed to half its original width but doubled in height, the

Thus we assume the gunner aims at a target about 9½ miles (months) ahead on the time road, but, like all gunners, sometimes overshoots and sometimes undershoots the mark.

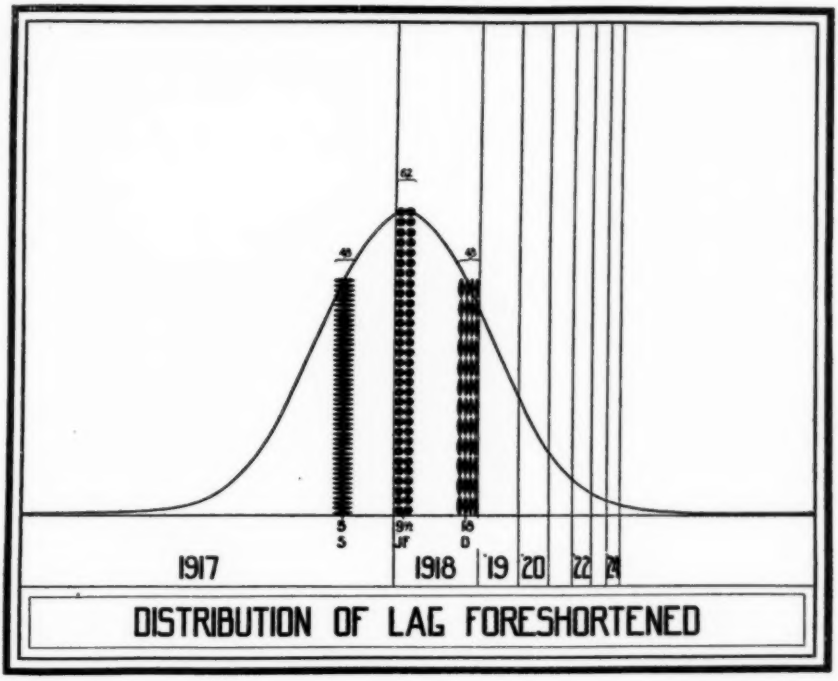


CHART 3

Showing Chart 2 transformed by substituting for each abscissa its logarithm (and representing each bullet pile by the same *area* as before). The letters S, etc., stand for September, etc., as in Chart 2. The numbers 5, 9½, 18 show how far these months are from April.

---

bullets being compressed horizontally. Reversely, for the fifth month's area, the bullets are stretched horizontally. In Chart 3, then, the law of distribution is the ordinary normal law: $y = P'e^{-\frac{x^2}{2\sigma^2}}$ in which x, the abscissa, is not time, but the logarithm of time, and is measured from the mode. The equations of transformation (from x and y, the coördinates of Chart 3, to X and Y, the coördinates of Chart 2) are as follows: $x = \log X - \log T$, and $ydx = YdX$, where T is the time (9½ months) from the gunner to the "target," and so log T is the abscissa in Chart 3 of its mode. From these equations we may derive, as the equation of Chart 2:

$$Y = P' \frac{1}{X\epsilon \frac{1}{2}\left(\frac{.6745\ (\log X - \log T)}{\log A}\right)^2}$$

in which T, the "target distance," is 9½ months, and A, the "accuracy ratio," is .53. Log A is .6745 σ where σ is the standard deviation of the normal curve in Chart 3.

Thus the two constants T (target distance) and A (accuracy ratio) fully determine the character of the distribution, while the total number of shots distributed is proportional to P'.

*By actual trial such backward distribution lessens the correlation.

The assumption of a *fixed* lag would mean, on the other hand, that the P' marksman invariably hit the target, or point on the road of time, at which he aimed. We may think of each height (ordinate) of the P' curve as a vertical pile of bullets to be shot forward on our time-road. If each such pile were shot *exactly* six months forward we would simply have a fixed six months' lag and the bullets, after being piled up again at the points where they strike—in each case six miles

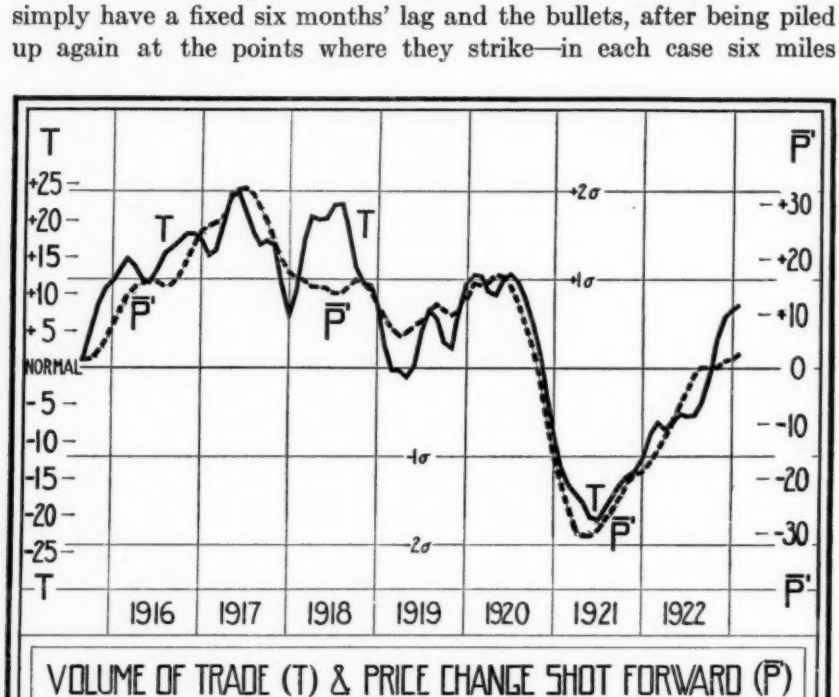


CHART 4

Comparing T (trade) for 1915–23 with $\bar{P}'$ (price-change shot forward. Each ordinate of $\bar{P}'$ is a bullet pile consisting of quotas shot forward from various P's.)

The scales of the two curves $\bar{P}'$ and T are such as to make their standard deviations coincide.

(months) further on—would simply form a duplicate of the original curve, as though the entire array of bullet piles were shifted bodily.

By a distributed lag the curve P', or array of shot piles, is shifted piecemeal, as it were. In the reconstructed curve (which we shall call $\bar{P}'$) formed by such shifting forward of P', and shown both in Chart 1 and Chart 4, any ordinate, or bullet pile, is clearly a blend. The result is that the total number of bullets piled up at say, August, 1917, consists not simply of a few of the 726 bullets shot from April, 1917, and shown

in Chart 2, but also of some bullets shot from May, some from June, some from July, as well as some from months prior to April, running back indefinitely.

When all these quotas are put together, the resultant piles—or rather the curve drawn through their tops—form the final curve $\overline{P}'$, shown in Chart 4. The $\overline{P}'$ is the same as that in Chart 1, except that it is plotted on a magnified scale. This new curve, $\overline{P}'$, considered as an array of bullet piles, is derived from the prior curve or array of bullet piles, P', simply by shifting forward all the bullets according to the principles of probability.

As has been shown the distribution of the lag, pictured in Charts 2 and 3, is simply the normal, symmetrical, probability curve, relatively to time when this time is subdivided into intervals not equal but constituting equal percentage increments—a geometric progression.

But such a normal distribution may take an infinite variety of forms differing in (1) target distance and (2) accuracy. Our problem is to find the *best* target distance and accuracy. This means the target distance and accuracy pertaining to that particular form of the normal curve which, when used for converting the curve P' into the curve $\overline{P}'$, will make this $\overline{P}'$ approximate T most closely.

What is meant by target distance has, perhaps, already been made sufficiently clear. In Charts 2 and 3 it is $9\frac{1}{2}$ months.

What is meant by gunner's accuracy may be indicated in various ways. On the symmetrical probability curve of Chart 3 we may locate the two "probable error" points equidistant from the mode and the two corresponding points on Chart 2, not equidistant, but equiproportioned. That is, if—on Chart 3—the *left* point is at September, 1917, five months from the gunner's position (at April, 1917), it is approximately half the target distance. More exactly, it is 53 per cent. The *right* point will be about October, 1918, *i. e.*, at about twice the target distance. (More exactly, the target distance is 53 per cent thereof.) This gives an accuracy of 53 per cent in the sense that the gunner is just as likely (when he falls short of the target at all) to fall short by more than 53 per cent as to fall short by less than 53 per cent while, likewise, he is just as likely as not, when he overshoots the $9\frac{1}{2}$ months' mark, to shoot almost double that distance. That is, the left probable error point is at 53 per cent of the target distance, while the target distance is 53 per cent of the distance to the right probable error point.

A 99 per cent accuracy would bring these two points very close to the target, while a 100 per cent accuracy would mean that the gunner

always hit the target. The accuracy ratio, 53 per cent, is actually found to be best.[1]

This same "law of the lag" applies to all the shooting, from whatever point on the road, the only [2] difference being in the number of bullets shot from the various stations, *i. e.*, the heights of P' at various points.

To review our course: we have obtained *three* curves in sequence, P, P', $\overline{P}'$. These are respectively, the original *price* curve, the *price-change* curve, and the *projected price-change* curve. The ordinates of the second are the slopes of the first. The ordinates of the third, considered as bullet piles, are formed by shooting forward over many months, each of the ordinates (bullet piles) of the second. In this shooting each original pile of bullets is scattered and redistributed according to the specific probability curve found.

In brief, P pictures the price level and, by implication, reciprocally, the purchasing power of the dollar.[3] P' pictures, in like manner, the "dance of the dollar"; $\overline{P}'$ pictures [4] trade or business as it would dance with the dancing dollar without including any other influence; while T pictures business as it *actually* dances, determined not only by the dollar's dance but by innumerable other causes as well.

RESULTING MAXIMUM CORRELATIONS

The correlation,[5] 94.1 per cent, is, as already stated (approximately), the maximum correlation obtainable, *i. e.*, it is greater than that obtained for any target distance and gunner's accuracy other than the 9 months and 52 per cent, or 9½ months and 53 per cent.

If, for instance, we try a slightly larger or smaller accuracy ratio, .52 or .54 (while keeping the target distance 9½ months), or a slightly larger or smaller target distance, 9 or 10 months (while keeping the accuracy ratio 53 per cent), we find a smaller correlation, in each case, as follows:

[1] A 9 months' target distance with 52 per cent accuracy gives, however, as good a result as a 9½ months' target distance with 53 per cent. Both give a 94.1 per cent correlation with T.

[2] At one time I had thoughts of trying the effect of making the target distance, or the accuracy, or both, a function of the rate of price-change. But I gave this up when it became clear, from the high correlation found, that such a variation in the target distance, or accuracy, could not appreciably improve the result, especially in view of the difficulty of finding the maximum when three or four variables enter. Even our two variables require a surprising number of calculations before the maximum correlation can be found. I am indebted to the Standard Statistics Corporation for defraying a large part of the expense of these calculations.

[3] This purchasing power is in wholesale trade only. For the purpose of this study, *i. e.*, to help explain and forecast trade, such purchasing power is doubtless more useful than purchasing power in any other sense.

[4] $\overline{P}'$ is smoother than T and should be so; for T is subject to many accidental disturbances while $\overline{P}'$ shows what T would be if the influence of price-change alone were at work and produced its effect according to the probability curve, which evidently must blend together, or smooth out, the impulses emanating from many different points of time.

[5] This is, of course, for *simultaneous* pairs of items, a January $\overline{P}'$ item paired with a January T item, a February $\overline{P}'$ with a February T, a March with a March, etc.

For	9½	months and	52%	94.04%
"	9½	" "	54%	93.98%
"	9	" "	53%	94.05%
"	10	" "	53%	93.86%
For	9½	months and	53%	94.10%

Such remarkable correspondence between T and $\bar{P}'$ for the period covered is exceeded for the part of the period, from December, 1918, to August, 1922, inclusive. The correlation for this period between $\bar{P}'$, the price-change forecaster, and T is 97.2 per cent.

These correlations are so high as to leave little or no doubt that changes in the price level afford an almost complete explanation of fluctuations in the volume of trade for the above period beginning in 1915 and ending in 1923.

Numerous other influences must naturally cause fluctuations in the volume of trade but they are of minor importance, or, rather, were so for the period 1915-23, here considered. The only important disturbance indicated in Chart 4 occurred in 1918, presumably representing the effect of government war orders.[1]

"THE" BUSINESS CYCLE A MYTH?

The conclusion that changes in price level almost completely explain fluctuations in trade, for the period 1915–23, seems to be of cardinal importance in relation to the so-called "business cycle." Is there such a thing as *the* "business cycle"? That there is little, if any, trace of it in the period 1915–23 is a fair corollary from the main conclusion just reached. With a correlation of nearly 100 per cent between trade and projected price-change, there is little left to explain.

Of course, if by the business cycle is meant merely the statistical fact that business does *fluctuate* above and below its average trend, there is no denying the existence of a cycle—and not only in business but in any statistical series whatsoever! If we draw any smooth curve to represent the general trend of population, the actual population figures must necessarily rise sometimes above and sometimes below this mean trend line. Otherwise the series would be as remarkable as the great orator who, his enthusiastic friend claimed, never fell below his average and often rose above it! In the same way weather conditions necessarily fluctuate about their own means; so does the luck at Monte Carlo. Must we then speak of "the population cycle," "the weather cycle" and "the Monte Carlo cycle"?

I see no more reason to believe in "the" business cycle. It is

[1] In Chart 5 it may be seen that the same sort of divergence between $\bar{P}'$ and T occurred in 1914–15, due, evidently, to the outbreak of war. There was also a divergence from 1889–92.

simply the fluctuation about its own mean. And yet the cycle idea is supposed to have more content than mere variability. It implies a regular succession of *similar* fluctuations, constituting some sort of *recurrence*, so that, as in the case of the phases of the moon, the tides of the sea, wave motion, or pendulum swing, we can forecast the future on the basis of a pattern worked out from past experience and which we have reason to think will be copied in the future. We certainly cannot do that in predicting the weather or Monte Carlo luck. Can we do so as to business? Not so long as business is dominated by changes in the price level! For changes in the price level show no regular recurrence, except possibly (1) a seasonal swing and (2) the eight-year swing worked out by Professor H. L. Moore to correspond to rainfall cycles.

In short, if the one *non-cyclical* or irregular factor, price-change, can so nearly explain the behavior of business, there is little room left for any *cyclical*, or regular, factors, especially as there must be numerous other non-cyclical ones always at work.

I do not mean to deny the possibility of *tendencies* toward regularity in trade oscillations. But these tendencies may always be defeated in practice, or blurred beyond recognition. Doubtless there lie hidden in the actual fluctuations of trade, as in those of weather, not only one wave movement or pendulum swing but many. My contention is that not a single one of such cyclical tendencies in business has yet been isolated.[1]

If I were to choose a physical analogue it would be not the swing of a clock pendulum but the swaying of the trees or their branches. If, in the woods, we pull a twig and let it snap back, we set up a swaying movement back and forth. That is a real cycle, but if there is no further disturbance, the swaying soon ceases and the twig becomes motionless again. In actual experience, however, twigs or tree-tops seldom oscillate so regularly, even temporarily. They register, instead, chiefly the variations in wind velocity. A steady wind may keep the tree for weeks at a time, leaning almost continuously in one direction and its natural tendency to swing back is thereby defeated or blurred. Its degree of bending simply varies with the wind. That is, the inherent pendulum tendency is ever being smothered. In the net resultant motion there is no perceptible trace left of real recurrences and no would-be forecaster would attempt to base his estimate of the future behavior of a tree-top on a statistical average period of recorded swayings taken in gusts of wind.

Another objection to the theory of cyclical regularity in business is

[1] I am here speaking only of actions and reactions such as are usually meant by those believing in "the" business cycle. I do not mean to contest the claims of Professor H. L. Moore in his brilliant book, "Generating Economic Cycles."

that it overlooks "friction." The twig, once deflected and then left to itself, soon stops swaying. So also a rocking chair, left to itself, will soon stop rocking; so also will a pendulum in a clock which has run down. Friction brings them to rest. To keep them going some outside force must be applied. So, in business we must assume that the effect of any initial disturbance would soon wear off, after a very few oscillations of rapidly diminishing amplitude. The resultant business cycle would speedily cease altogether if dependent only on its own reactions. To keep it up there must be applied some outside force. But, unless the outside force happens also to be cyclical and unless, in addition, the rhythm of said force or forces happens to be exactly synchronous with the business pendulum itself, these outside forces will not perpetuate, but obfuscate, the cycle, like the wind blowing on the trees. We cannot imagine anything analogous to the "escapement" in a clock which so nicely times the outside force as to keep up the natural swing of the pendulum.

I, therefore, have no faith whatever in "the" business cycle. I do not doubt that, after any disturbance in one direction or the other, business tends to swing back to normal (and a very little beyond) just as does the tree. But will it? Not if the wind still blows to prevent it. I do not think there is much real utility in estimating the area of abnormality on one side of normal in order to predict an equal area to follow on the other, nor in assuming that the business oscillations will follow a ten year periodicity or a forty two months' periodicity[1] or any other. Economic statistics are, as yet, miles away from any real knowledge of even these natural tendencies to periodicity *in vacuo*,

[1] In fact, any exact periodicity or equality of time intervals between successive recurrences in "the" cycle have been very largely abandoned even by the most ardent protagonists of the cycle. Professor Mitchell in his *Business Cycles* even goes as far as to say (p. 581), "As a matter of fact these cycles differ widely in duration, in intensity, in the relative prominence of their various phenomena, and in the sequence of their phases. . . . In view of these diversities, the notion that crises have a regular period of recurrence is plainly mistaken." But when "the" cycle has been thus emasculated, what is left of it which *is* "cyclical"? It must be remembered that seasonal variations, which often do show a cyclical tendency, are first eliminated as is also the secular trend. What is then left seems unworthy to be called a cycle. Practically its only claim to such a title is due to the elimination of the secular trend. This elimination creates an artificial appearance of oscillating even when the original figures are all increasing or all decreasing. But, unless the resultant ups and downs, *relatively to the trend*, show some similarity other than the bare truism that they do fluctuate about their own mean trend, the "cycle" is in no way different from any other series of figures. In fact, the elimination of the seasonal cycle leaves even less reason for calling the residuum "the" business cycle. It is found to consist very largely of irregular variations due to price changes. What is still left may possibly consist largely of Moore's eight year "generating" cycles, also imposed from outside and not a matter of internal action and reaction. The residuum still left should be very small and, besides containing numerous irregular variations, may very likely contain a whole series of cyclical elements not yet isolated. What I object to is not the idea that such cyclical *causes* are at work, but the idea of calling the resultant irregular final *effect* of such a bundle of unanalyzed causes, "the" cycle or even "a" cycle.

As to dictionary meanings, the usual meaning implies periodicity, and every meaning implies *some* sort of definite recurrence, presumably beyond that of mere fluctuation about its own mean trend to which every statistical series is subject.

and when, if ever, we do attain to such a knowledge we shall find not a clock pendulum but a bough of a tree, in the grip of outside forces of which the chief for 1915–23 was the rise and fall of the price level, the dance of the dollar. This dance in turn is controlled only slightly by the pendulum principle but chiefly by extraneous forces such as government inflation policies, banking policies, legislation, foreign trade, gold mining, and many other scarcely recurrent, and certainly not periodic factors.

The indications from this study are that, as long as price levels change greatly, from whatever cause, "trade" will vary to match.

Of course it is also true that, if ever we have a stable dollar, *i. e.*, a stable price level, the smaller oscillations of business then remaining will have to be explained by other causes than unstable money. Possibly then, when the monetary winds have ceased to blow, we shall be able to observe, in the ensuing calm, our business tree-top sometimes gently swaying back and forth in its natural swing, or composite of swings.

RESULTS TESTED BY OTHER PERIODS

We have interpreted the high correlation found as indicating a causal relationship. It may also be interpreted as an anticipatory relationship. It may be that a rise in the price level indicates an expectation of better business while a fall indicates an expectation of depression. Probably there is some truth in this view, although it is diminished greatly by the fact that a *general* rise or fall of prices is less apt to be anticipatory than the rise or fall of an individual price relatively to the general level. But, from the standpoint of the utility of the relationship in practical prediction, it makes little difference whether it be causal or anticipatory; the correlation certainly exists and is remarkably high. $\overline{P}'$ and T fit each other closely for the period studied.

But the mere fact that in this period of great price disturbance a good fit has been found does not of itself prove that the fit will be equally good for all other periods. This should, in fact, not be expected, and for two reasons: (1) because the factors other than monetary instability will usually be of more relative importance than for the price-disturbed period here studied and (2) because the true law connecting P (via P' and $\overline{P}'$) on the one hand, and T on the other, is probably not *exactly* disclosed to us by the experience of the period covered, 1915–23.

Let us consider reason (1) first. As already stated, the period taken, 1915–23, was chosen chiefly because the factor of price-change was so conspicuous. It stands to reason that, in such a period of large and rapid fluctuations in the price level, this one great factor would throw all others into the shade. The statistical fit found in such a period

would therefore come nearer to expressing the true relationship between P and T which we are seeking to discover than in a period of relative price stability. When, however, we come to apply this relationship, even though it were the *absolutely* true one, to another period in which price-change does not so dominate, the correlation between the $\overline{P}'$ and T could scarcely be expected to be so great. Evidently in the extreme case where the dollar does not vary at all, the curve $\overline{P}'$ would be reduced to a horizontal straight line. In that case the fluctuations still left in T would be only those resulting from other influences than price-change.

Coming to reason (2), we must recognize the fact that the statistical fit between $\overline{P}'$ and T which has been found for the particular period 1915–23 is, in all probability, not quite free of extraneous taint. It assumed that all extraneous influences, *i. e.*, all influences other than that of price-change, cancel themselves out. We were forced so to assume. That is, in the absence of any knowledge of the direction in which these extraneous factors disturb our figures, we had to assume that they did not disturb them at all. But these extraneous factors would undoubtedly have *some* effect on our calculations so that our target distance and gunner's accuracy used in obtaining the maximum correlation probably do not exactly express the true law of the lag, even assuming as absolutely correct the normal probability curve, geometrically applied.

A good test would be to determine whether the law as found applies to other periods than the one from which it was found.

Unfortunately we have no other period available in which the price-factor is variable in any such degree as in the disturbed period, 1915–23. We can, however, divide that period into two parts, ascertain the law best fitting one of them, and then try it on the other.

These two parts are: (1) the most fluctuating part (from December, 1918, to August, 1922, inclusive), and (2) the remainder. If, now, we apply the law ($9\frac{1}{2}$, .54) obtained from "the most fluctuating part" to "the remainder" period (which played absolutely no part in deriving that law), we find a correlation of 74.90 per cent which is nearly as high as the maximum (75.62 per cent) obtained from that period itself. Conversely, if we apply the law ($9\frac{1}{2}$, .52) obtained from "the remainder" period to "the most fluctuating part," we find a correlation of 97.19 per cent—nearly as high as the maximum (97.28 per cent) obtained from that period itself.

In short, while the 97.28 per cent and 75.62 per cent may be overestimated because we purposely selected the target distance and accuracy which would make them high, certainly this cannot be said of the

nearly as high figures, 97.19 per cent and 74.90 per cent, found for each period by applying the law of the other period.[1]

We conclude that it makes very little difference to our estimated law of the lag whether it be obtained from one period or another; and the slight difference it does make affects but slightly $\overline{P}'$ and its correlation with T.

The best estimates would seem to be the $9\frac{1}{2}$ and .53 for the whole period. The period December, 1918, to August, 1922, is too short, which fact seems to offset its advantage in amplitude of oscillation, although, as noted, the results obtained are practically the same in any case.

But it may be objected, as was suggested at the November meeting of the Statistical Association, that even the whole period (1915–23) is too short to afford convincing evidence of the reality of the relationship between $\overline{P}'$ and T. May the correspondence not be a coincidence?

To answer this question, I have applied the results found above to all other periods for which we have data at all comparable to those already used. The results ought to satisfy the most hard-headed sceptic both as to the reality of the relationship and as to any possible vitiation through selection. There can be no suspicion of selection; for the target and accuracy employed in calculating correlations between $\overline{P}'$ and T of these other periods are obtained, not from these periods themselves, but from the period already studied (September, 1915, to February, 1923), and have not been varied to suit any other period.

Chart 5 shows $\overline{P}'$ and T from 1877 to the present time.[2]

First, taking the period 1923–24 subsequent to the main period studied, we find a correlation of 78 per cent.

Prior to 1915, we have statistics of wholesale prices (P) and of the physical volume of trade (T) both back to 1900 by months. For the period, 1903–15, the correlation between $\overline{P}'$ and T is found to be 67 per cent. Using the index of the American Telephone and Telegraph Company,[3] we find 77 per cent.

We cannot go back of 1903 by months. But quarterly[4] figures are available, those for the price index (J. L. Snider and Falkner) beginning in 1861, and those for the trade index (American Telephone and Tele-

[1] Nor are these figures very different from the 97.24 per cent and the 75.52 per cent (found by applying the law derived from the whole period, $9\frac{1}{4}$, .53).

[2] The break between 1899 and 1903 is due to the lack of an index number of prices for the last *quarter* of 1899, which lack prevents the calculation of a P' for several quarters, and thus impairs the calculation of $\overline{P}'$ until 1903. Nor can the *monthly* price index that begins in 1900 be converted into $\overline{P}'$ until 1903 In Chart 5 the standard deviations of $\overline{P}'$ and T are not equated (as in Chart 4).

[3] This index differs from the Harvard Index in that it includes Bradstreet's Wholesale Price Index (dropped after 1913), Bradstreet's Index of Failures, and railroad gross revenues, but is like the Harvard Index in that it includes pig iron production.

[4] The necessity of using for this period a quarterly, rather than a monthly, correlation, probably does not appreciably lower the results obtained, since this quarterly method applied to the other periods lowered the correlation only by about 5 per cent.

graph figures for blast furnace capacity) beginning in 1877. These show for 1877–99, inclusive, a correlation of 58 per cent ($r = .579 \pm .047$).

It has, then, been definitely demonstrated that there exists a close relationship (between changes in the wholesale price level "shot forward" and the volume of trade described) for the whole period, 1877–1924, a period of nearly half a century, and for all subdivisions of that period which have been tested except three. These exceptions are: (1)

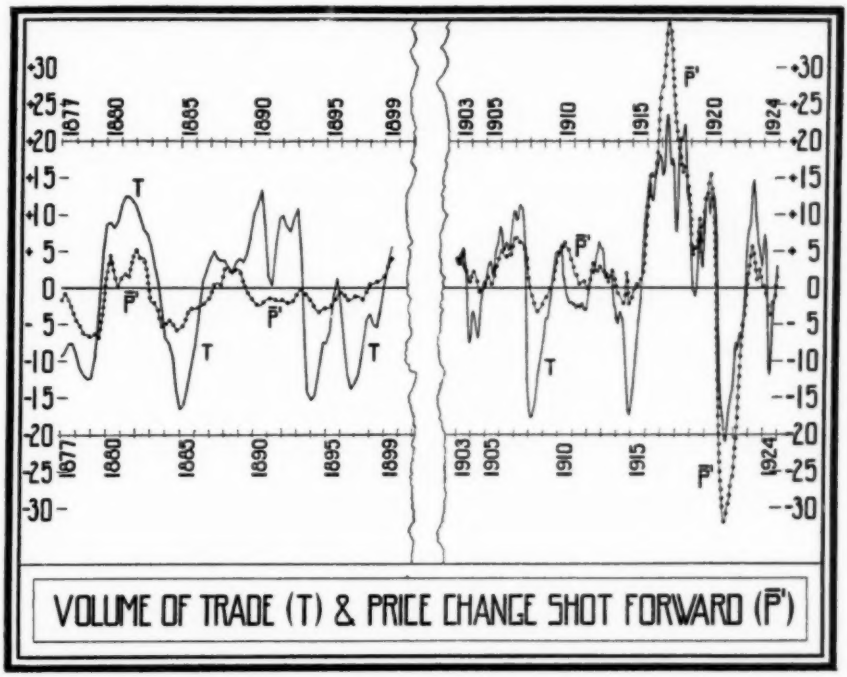


CHART 5

Comparing T and $\overline{P}'$, 1877 to 1924. Based on Chart 4, but with different scales, and calculated by the simplified method described below.

1889–92; (2) July, 1914, through August, 1915; (3) January, 1918, through November, 1918. The last two confirm rather than contradict the main conclusion. The correlation for period (1) is *minus* 84 per cent. In period (2), June, 1914, to August, 1915, trade barely follows price-change, being deflected by the unexpected outbreak of war. The correlation for this period is plus 25 per cent. During period (3) the great volume of governmental war orders (which was, of course, dependent on governmental policy and wholly irrespective of prior price-changes) might naturally be expected to out-influence any other factor.

For this period the correlation between $\bar{P}'$ and T works out as *minus* 91.1 per cent. For all other periods price-change puts its stamp on "trade," although in every case, of course, other factors have some influence.[1]

REMAINING PROBLEMS

The relative importance of price-change and these other influences, is difficult to measure. But Chart 4 shows that it is possible to make $\bar{P}'$ fit T so closely that the deviations still remaining between them are very small compared with the total variations of either.

Price-change is not here offered as a complete explanation of the fluctuations of "trade." No explanation could be absolutely complete. But it should be possible materially to reduce the unexplained residuum by making further studies (using a distributed lag) for other known factors. For instance, next to the influence of price-change the most important is probably that of the rate of interest.

Until recently I had the idea that these two could best be considered in combination, as the *real* rate of interest, *i. e.*, the sum of the rate of interest in terms of *money* and the rate of appreciation (positive or negative) of the purchasing power of the dollar.[2] But I am now inclined to believe that the two factors can best be studied separately because, for one reason, the rate of interest (in money) is always perceived while the rate of appreciation of money is not.

A study might well be made to discover the law of influence of the

[1] All of the correlation coefficients above have been calculated by using the probability distribution of lag already explained. At the November meeting of the *Statistical Association* it was suggested that, to make the method more practical, a short cut should be devised. This can be accomplished by using a straight line to approximate the curve of Chart 2. By this method the influence of any given price-change is assumed to begin at a maximum immediately (*i. e.*, at the very next month or quarter following the given price-change) and then to taper off by equal reductions for each successive interval of time. This method will save labor especially when a maximum correlation is sought. To determine the *two* parameters, target distance and accuracy, which give the highest correlation, a great deal of labor is required. But by having only *one* parameter to vary, as we do in the linear tapering off now considered, the labor is reduced by at least four-fifths. The only variable becomes the number of months in which the influence tapers off to zero. Much to my surprise (and possibly indicating that the type of distribution of Chart 2 is not the very best), this linear or triangular distribution seems to give practically as high a correlation as the more strictly probability distribution (if not higher). Thus, for the standard period 1915–23 by months the linear distribution which gives the maximum correlation is that in which the tapering off to zero requires *25* months. The correlation between the resultant $\bar{P}'$ and T is 95 per cent, as compared with 94 per cent by the longer, or Chart 2, method. For 1923–24 the linear method gives a correlation of 92 per cent *versus* 78 per cent by the regular method. For the period 1903–15 the linear method gives 79 per cent *versus* 67 per cent.

By quarters the nearest equivalent distribution (*i. e.*, nearest to the Chart 2 method) covers eight quarters. Between 1877 and 1899, a correlation by quarters between $\bar{P}'$ and the American Telephone and Telegraph Co's Index of Trade gives 60 per cent by the linear method, compared with 58 per cent by the regular method.

The correlation for 1903–24 is 81 per cent.

Such a short-cut method of distributing a lag may prove the most practical for general use in statistics of any kind.

[2] See my *Rate of Interest*, 1907, Chapters 5, 6, based on my "Appreciation and Interest" (*Publications of the American Economic Association*, Vol. XI, No. 4, 1896, p. 410).

rate of interest on business, using the same principle of a distributed lag here employed but selecting as a period of study one in which the price level was comparatively stable.

With such an interest law and with the price-change law here set forth, it may be possible to combine the two influences by simple addition and explain the so-called "business cycle" more fully than by either separately. Possibly then a third factor may be added, and so on, until we have a nearly complete, as well as a rational, statistical explanation of the behavior of business conditions.

It would seem that the studies hitherto made of the so-called business cycle are too empirical. Mere empiricism is seldom very fruitful. The present study aimed to begin with a rational analysis and then to test and measure statistically, rather than to begin with actual historical "cycles." We thus isolate one of its innumerable factors. After several such—price-change, interest-rate, etc.—have been isolated and measured, they may be combined.

We ought then to be in a position to predict business conditions in a truly scientific manner, basing those predictions on known data of price-change, interest-rate, etc., much as we forecast the weather; and without necessarily any cyclical variations to deal with.

In a similar way, we may go still further back and analyze the influences statistically determining the price level, as well as those determining the rate of interest and all the rest.

Monetary theory ought, for instance, to help us analyze and predict the price level. Professor Alvin Hansen and Professor Holbrook Working have already pointed out that, conformably to the quantity theory, the price level follows bank deposits with a few months' lag.

The lag, Professor Working tells me, that gives the highest correlation is six months.

In such ways we may be able not only to forecast the volume of trade by certain statistical forecasters, such as price-change, but also to forecast these forecasters.

And, besides thus tracing back the ancestors, so to speak, of trade, we may trace its descendants (not to speak of its brothers, cousins, and other relatives) such as unemployment,[1] bankruptcies, and all other statistical series, bound up with trade.

Finally, we should be led to control and reduce the so-called "business cycle." The present efforts of the Federal Reserve System to prevent periods of speculation constitute a rough stabilization of the dollar and should go a long way toward ridding business of what has so often been its greatest disturber, our unstable dollar.

[1] The "T" used in the present study includes employment.

SUMMARY AND CONCLUSIONS

1. Not the price level itself (P), but its rate of rise or fall, (P') varies with the volume of Trade (T), (P and T being taken in the restricted sense explained in the text, and T being not absolute but *relative to secular trend*).

2. If the curve P' (plotting the rate of change of P) is shifted forward *bodily* by 7 months, its correlation with the curve T is 73 per cent. This "fixed," 7 months', lag gives a higher correlation than any other "fixed" lag.

3. But if P' is shifted forward *piece-meal*, the correlation may be greatly increased. We may conceive each month's P' as a column of bullets which, instead of being shifted bodily, are shot forward individually to different distances or lags. Such a distributed lag may be conceived according to any rule desired. The rule finally adopted as best for this study is somewhat analogous to the laws of probability according to which a gunner would distribute a bullet pile (P') by shooting each bullet of that pile at a target on the road ahead.

The particular probability distribution assumed is the "normal" one, but applied geometrically (*i. e.*, "normal" relatively to the logarithm of the distance ahead—or *time*—from the gunner).

4. The above is the *general* law of the lag adopted. It admits of variation in two respects: (1) as to the "target distance," and (2) as to the gunner's "accuracy."

A target distance of $9\frac{1}{2}$ months means that a bullet is more likely to strike the point $9\frac{1}{2}$ months ahead (or rather within, say, one per cent of that distance) than to go any other given distance (*i. e.*, within one per cent of *that* distance).

An accuracy ratio of 53 per cent means that any "bullet" falling short of this target is just as likely to fall between 0 per cent and 53 per cent of the target distance, as it is to fall between 53 per cent and 100 per cent (and, similarly, any bullet overshooting the target is just as likely to fall between a point of whose distance the target distance is 53 per cent, as to fall beyond that point).

5. Evidently the bullets which reach any particular point on the time road will not now (as they did with a fixed lag) consist exclusively of those shot from the point seven months back. Only a few will come from that source; while some will have come from six months, some from eight months, etc. The total reaching that particular point is thus a composite made up of small quotas from many sources. This total is called $\overline{P}'$.

6. We now correlate $\overline{P}'$ (*i. e.*, P' shifted forward piece-meal) with T,

and find that the correlation for the period 1915–23 is made a maximum (94.1) by using a target distance of 9½ months and an accuracy ratio of 53 per cent.

7. As this period, 1915–23, seems to be better adapted than any other for estimating the two magnitudes (target and accuracy), these values (9½ and 53) are assumed to be the best estimates of their true values, *i. e.*, give us the best law of distribution of the lag.

8. The possible objection that these figures, 9½ and 53, are selected or fitted to the particular period considered has little force; for substantially the same results are obtained when such selection is avoided.

Thus, if we take the lag which best "fits" one period and apply it to a totally different period, it yields almost as high a correlation as does the lag which is especially fitted to that (second) period.

9. Accepting 9½ and 53 (or, alternatively, 9 and 52 which yield the same result) as the best estimates, we find a high correlation for *every* period studied, as follows:

(1) 1877–99	+58%
(2) 1903–15	+67%
(3) 1915–23	+94%
(4) 1923–24	+78%

10. In three cases, short portions of the above periods seem inconsistent with the general conclusion: (1) 1889–92 when the correlation is −84 per cent, (2) 1914–15 (at the end of the second period) when the correlation is + 25 per cent, and (3) 1918 (during the third period) when the correlation is −91 per cent.

Two of these three apparent exceptions confirm rather than contradict our conclusion (that P, *via* P' and $\overline{P}'$, has a strong influence on T). During these exceptional periods special war influences overshadowed all others: namely, 1914–15 at the outbreak of war, and 1918, when American war orders swelled T.

11. The high correlations found for the other years indicate that $\overline{P}'$ dominates T, especially when P fluctuates violently.

12. During all the periods here studied, and especially 1915–23, the so-called "business cycle" for T would seem to be largely mythical, since the influences to which we can very largely ascribe the fluctuations in T—namely P (*via* P' and $\overline{P}'$) and special war influences—are none of them known to be very cyclical or rhythmic, in their fluctuations.

Probably much of the remaining fluctuation of T, not explained by the factors here studied, is due to non-cyclical factors also. There can be little left in the fluctuations of T which can be said to be truly cyclical in character.

In that residuum, however, it is not only possible but altogether probable that cyclical components, not simply one but many, will be found.

But the final resultant, T, a composite of numerous elementary fluctuations, both cyclical and non-cyclical, is probably seldom very clearly cyclical itself. Certainly to call this resultant "the" business cycle is misleading.

ON THE MEASUREMENT OF SEASONAL VARIATION

By O. Gressens, *University of Illinois*

A great deal of attention has recently been given to the study of a method for measuring seasonal variation in economic data. This factor, though easily defined, yet creates a considerable controversy in respect to a method for measuring its effect, the fundamental question being: To what extent and in what manner can averages be employed in measuring seasonal variation?

In the case of a time series averages have an equivocal value. Inferences which can be drawn from them are valid only for the period over which they were calculated, and any projection of fixed averages into the future must be made upon the assumption that all variable factors remain unchanged, a static condition never realized in economic society. The dynamic forces which are continuously changing conditions give expression to these changes in the changing averages of time series. Every addition of items to such series changes the position of the averages, so that with economic data they must be used with extreme caution. If the period under consideration covers a considerable span of time, averages calculated over the entire period have only a remote bearing on the conditions at any one particular time or on any fraction of the period. The significance of the general averages when applied to a fractional period is therefore questionable.

In the case of series which can properly be dealt with by the theory of probability, where all the possible events are known in advance, the importance of averages of samples can be ascertained. They are, therefore, in these cases not only a convenient method of analysis, but a most illuminating form of expression. With time series, however, averages calculated over an extended period are meaningless for immediate purposes, and become increasingly so as the period over which they are calculated is lengthened. This fundamental characteristic is recognized in calculating the "normal" of time series. "Normal" is defined not as a static condition but as a continuously changing position, either constantly moving to higher or lower levels. The continuous changes to which time series are subject are clearly shown in their statistical analyses both in the long-time trends and in the sequences of cyclical fluctuations which are measured from the trend as the normal position.

For the measurement of the seasonal characteristic, however, these considerations are usually disregarded. Most of the methods developed for calculating the effects of seasonal variation are based either on long-

time averages, usually calculated over the entire period under consideration, or on fixed averages used as bases. The discussions have in the main not been as to the real nature of the seasonal swing, but rather as to the respective adequacy or inadequacy of certain types of statistical averages. Whenever a single set of indices, however, is used to express the seasonal movement the fundamental assumption is that the normal seasonal is an unchanging factor for the period over which it is calculated. To quote Professor Persons:[1] "Although we wish to ascertain if a systematic variation exists it is not accurate to think of seasonal variation (or, for that matter, the other types of fluctuations) as being exactly the same year after year." Nevertheless Professor Persons concludes that if data are gathered over a sufficient number of years, a normal seasonal can be ascertained which can be expressed in a single set of fixed averages. In other words, though continuous changes are recognized in the other factors which affect time series, the normal seasonal influence is taken to be a fluctuation which occurs unchanged year after year with mathematical precision.[2] Professor Davies and Dr. Lincoln Hall,[3] among others, base their methods on the same principle. Professor Falkner [4] has departed somewhat from the usual procedure by averaging only the typical items of a central group, pointing out that the limitation of this method necessarily is a definite tendency of the respective monthly ratios to the ordinates of secular trend towards a central grouping.

The normal seasonal, however, cannot be expressed by a fixed set of numbers. Natural variations take place in the seasonal through shifts in demand due either to an actual change in demand which would bring about a permanent change in the seasonal, or to changes in demand due to the relative positions in the business cycles, which would cause temporary changes in the seasonal. Normally, therefore, the seasonal is a changing factor, just as the trend and the cycles are changing factors, and it cannot therefore be expressed by fixed averages.

There have been two recent proposals taking this circumstance into account. The methods are the one proposed by Dr. King [5] and the one proposed by Dr. Snow.[6]

Dr. King, in his method which he designates as "A New and Improved Method for Measuring Seasonal Variation," ignores the rigorous mathematical approach. He justifies this departure by the tenable argument that no simple mathematical curve could give even an ap-

[1] *Review of Economic Statistics*, Volume I.
[2] *Economic Statistics*.
[3] "Seasonal Variation as a Relative of Secular Trend," this JOURNAL, June, 1924.
[4] This JOURNAL, June, 1924.
[5] This JOURNAL, September, 1924.
[6] *Journal of the Royal Statistical Association of London*, March, 1923.

proximate fit to the original data, while the free-hand curve, which he calls the preliminary cycle curve, when manipulated by a person reasonably acquainted with graphic statistics, yields a better fit than would a highly involved mathematical curve.

This position of Dr. King's is sound. His series of moving seasonal indices calculated from the free-hand curve, predicating that the curve is well drawn, admit of a measurement of the seasonal without resort to the device of a fixed set of averages. They take into consideration the shifting propensities of the seasonal due to the various factors which change economic conditions. The fundamental contribution of Dr. King's method lies in the explicit recognition which is given to this characteristic.

In the application of this principle, however, Dr. King has developed a method which will not yield comparable results as between different manipulators. The personal equation, at best a difficult factor to eliminate, is allowed free play. In drawing the preliminary cycle curve, which forms the basis for determining the seasonal movement, the manipulator must rely entirely upon his own judgment, and just as his judgment is either good or bad, his results will be either good or bad. The mechanics of a method ought, however, to be such that with an exact application different manipulators can be assured of at least approximately comparable results.

In a paper published in the March, 1923, issue of the *Journal of the Royal Statistical Association*, Dr. Snow briefly suggests a method for the simultaneous treatment of the seasonal factor and secular trend which implies the same concept of the seasonal as that explained by Dr. King. Dr. Snow, after attacking the method of the Harvard Committee of Economic Research[1] as being cumbersome and logically not clear (the criticism, however, not being explained), proposes a simple and direct device for taking care of the seasonal factor in the analysis of economic data. He proposes to break up the year into comparable periods (all Januaries, all Februaries, etc., or all first quarters, all second quarters etc., being included in separate series), and to calculate the fitting trends for the separate series. If the calculated trends for these series are parallel, Dr. Snow concludes that the seasonal has been constant throughout the period under observation. Obversely a divergence from parallelism denotes a corresponding variation of the seasonal.

Dr. Snow's treatment of the seasonal is unique. It has the virtue of simplicity and directness, and, as does Dr. King's method, takes into consideration the yearly variations of the seasonal. It has the defect, however, of not presenting an isolated measurement of the seasonal,

[1] Persons, *Review of Economic Statistics*, Volume I.

which for some purposes is desirable. Also by the simultaneous elimination of these factors the precise effects of either of them are obscured. In cases where it is desirable to correct for seasonal variation only, Dr. Snow's method is not available.[1]

A simple and direct way to get an idea of the nature of the seasonal swing is to calculate the ratios of the monthly items to their own annual total or their own annual average month. If a frequency distribution of these crude ratios is then drawn up, a definite graphic picture of the seasonal variation is obtained. A close grouping of these ratios for the respective months will show a rather steady seasonal, while a tendency of the ratios to spread indicates a corresponding tendency of the seasonal to shift. An unintelligible scatter of the ratios without any discernible movement of the ratios from month to month may safely be regarded as evidence of the absence of seasonal variation.

In case the series dealt with exhibits the influence of secular trend only in a moderate degree, these crude ratios have, by the nature of their calculation, been sufficiently corrected for trend. For by referring the monthly items of each year to the annual total or the annual average of that year, the effect of the trend has been practically eliminated. For series, however, which definitely exhibit a trend influence, these ratios, though corrected for the annual increment due to trend, are not sufficiently corrected for the monthly increment. This can be made clear by the following example taken from the Gross Electric Sales figures of the Public Service Electric Company of New Jersey:[2]

	Ratios to Average Month		Ratios to Monthly Ordinates	
	1904	1905	1904	1905
January	110.77	111.44	105.88	112.54
February	100.38	97.37	95.26	97.70
March	95.77	92.81	89.89	92.21
April	91.92	90.52	85.66	89.35
May	86.54	87.25	79.79	85.30
June	87.69	87.25	80.00	84.76
July	85.77	86.27	77.70	83.02
August	90.00	90.20	80.69	85.98
September	96.15	96.41	85.62	91.33
October	108.07	107.52	95.25	100.92
November	120.00	122.88	105.05	114.63
December	127.69	130.29	110.67	120.54

Observing in the above table of crude ratios the respective December to January sequences it will be noticed that under the "Ratios to

[1] Professor Persons (*Review of Economic Statistics*, Vol. I) has criticised the methods proposed by M. T. Copeland and the Federal Reserve Board for first assuming that seasonal variation is present, and then assuming that all non-seasonal factors cancel each other. It can be added that these methods do not take into consideration the periodical variation of the seasonal.

[2] *Harvard Business Review*, July, 1924.

Average Month" the December, 1904, figure is 127.69, and the January, 1905, figure is 111.14. Under the "Ratios to Monthly Ordinates" the December, 1904, figure is 110.67 and the January, 1905, figure is 112.54. The conclusion is that the ratios to the average month of each year are corrected for the annual trend increment, but not for the monthly trend increment, because the original data are influenced by a very pronounced upward trend movement. The ratios calculated on the basis of the average annual month can be corrected closely for this monthly trend increment by deducting the product of the distance from the middle of the year by the ratio of the monthly increment to the average month of that year forward through December, and adding these percentages back through January. However, an inspection of the graph of the original data would reveal whether the data were subject to a pronounced trend influence, and in this case the ratios to the monthly trend ordinates can be immediately calculated.[1]

These ratios, however, still contain the effects of spurious fluctuations and the overlapping influences of cycles where the cyclical fluctuations do not coincide with calendar years. If these respective monthly ratios are taken, therefore, and the unusual items which could not be sufficiently subdued by a systematic smoothing process are moderated with regard to the items immediately juxtaposed, and the ratios are then systematically smoothed and adjusted, they will be corrected for these fluctuations. These sets of Adjusted Monthly Ratios yield measures of seasonal variation based upon the consideration that yearly fluctuations take place in the seasonal.

A concrete example of this method was worked out with the Gross Sales (Electric) of the Public Service Electric Company of New Jersey.[2] In determining the seasonal variation the following calculations were made:

1. The original data having been charted, it was evident on inspection that the secular trend of the sales had a decided upward tendency. The ratios calculated were therefore the percentages of the actual monthly sales to the monthly ordinates of the secular trend. (In this case it was found that three lines calculated by the method of least squares for the periods 1904–1909, 1910–1915, 1916–1923 gave the most satisfactory trend.) These crude or unadjusted ratios appear in Table I.

2. Two smoothing processes were then tried for three, five and seven items—a moving average and a moving median. It was found that in this case a five-item moving median gave the best results. This moving median smoothed the ratios sufficiently so that the successive ratios

[1] Falkner, Helen, this JOURNAL, June, 1924.

[2] *Harvard Business Review*, July, 1924.

of the respective months no longer evidenced the effects of spurious items or of overlapping cycles.

TABLE I
RATIOS OF MONTHLY SALES TO ORDINATES OF SECULAR TREND

Year	January	February	March	April	May	June
1904	105.88	95.26	89.89	85.66	79.79	80.00
1905	112.54	97.70	92.21	89.35	85.30	84.76
1906	121.62	105.65	94.69	91.79	84.30	86.13
1907	125.82	104.63	95.66	96.51	92.00	88.86
1908	113.16	101.26	91.25	87.59	81.23	80.88
1909	114.79	103.26	93.74	91.47	83.03	85.65
1910	119.69	107.61	98.71	95.34	88.49	89.05
1911	122.48	109.04	100.33	94.10	88.32	88.63
1912	121.73	105.00	99.56	96.08	90.44	83.92
1913	122.72	104.19	93.42	95.74	88.65	86.42
1914	122.10	108.39	97.30	98.40	90.61	87.01
1915	115.32	103.71	106.73	94.63	89.23	90.75
1916	118.39	109.38	106.77	97.76	92.32	92.65
1917	119.39	102.58	99.57	93.91	93.92	91.74
1918	97.80	88.61	101.61	97.59	93.37	91.59
1919	115.49	102.39	108.92	93.00	88.52	85.11
1920	122.12	105.25	97.45	99.45	90.44	91.11
1921	114.66	102.42	99.51	96.45	85.65	82.48
1922	116.75	101.47	98.51	92.48	84.99	83.33
1923	121.35	107.01	105.13	99.48	90.98	86.57

Year	July	August	September	October	November	December
1904	77.70	80.69	85.62	95.25	105.05	110.67
1905	83.02	85.98	91.33	100.92	114.63	120.54
1906	81.37	85.47	90.11	103.36	114.48	124.03
1907	84.47	88.22	96.89	108.01	116.41	123.16
1908	77.37	79.18	85.10	95.69	108.55	115.57
1909	78.51	82.21	89.93	99.55	113.94	120.04
1910	81.43	85.08	91.82	101.38	116.76	120.23
1911	80.71	84.78	92.12	103.12	114.41	119.02
1912	80.89	84.07	91.88	104.95	112.75	119.94
1913	83.09	84.23	93.38	106.29	112.62	121.32
1914	85.16	85.01	91.14	102.80	107.59	117.06
1915	86.74	88.23	96.27	104.68	113.53	122.37
1916	85.07	89.01	92.36	101.46	110.27	120.94
1917	85.40	85.68	88.91	104.28	106.71	113.71
1918	90.94	92.46	95.00	102.04	104.80	120.49
1919	83.07	85.48	88.60	102.13	105.76	121.11
1920	88.67	92.32	91.31	101.08	108.32	120.53
1921	77.09	77.94	82.08	96.03	106.68	119.02
1922	78.65	83.84	92.93	105.91	113.33	114.48
1923	79.98	83.83	85.92	104.96	111.78	122.42

3. The resulting smoothed ratios were then examined for any unusual items—spurious items which were not moderated by the smoothing process, and these items then moderated with regard to the immediately preceding and succeeding items of the respective months.

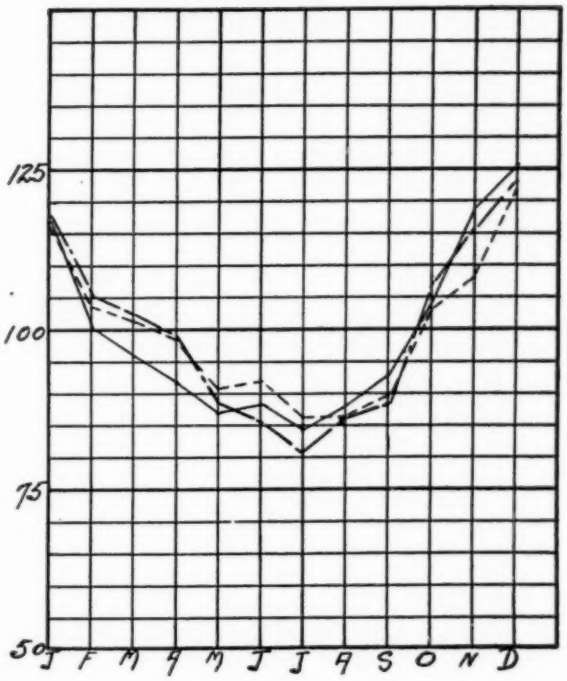
4. In extending the smoothed ratios backward and forward to supply the loss of data the following calculations with the extreme items were made: The middle item of n, n, $n-1$, $n-2$, $n-3$, etc. (In case of a moving average the calculation would be:

$$\frac{n+n+(n-1)+(n-2)+(n-3)}{5 \text{ (or } 7\text{, etc.)}},$$

where n = extreme item.)

5. The ratios were then adjusted to a common base within each year, so that the sums of the ratios for each year equal 1200.00. The resulting Adjusted Monthly Ratios appear in Table II.

Several years of seasonal indices are plotted on the accompanying chart, illustrating the variation in the seasonal.



This method for measuring the seasonal factor is based on the fundamental fact that yearly variations take place in the seasonal, and that therefore the seasonal is a varying factor just as the secular trend or the cycle fluctuations. In this respect it is in agreement with the methods developed by Dr. King and the method suggested by Dr. Snow. It is mechanically more definite than Dr. King's method, however, and it gives an isolated measure of the seasonal which Dr. Snow's suggested treatment does not.

TABLE II

ADJUSTED MONTHLY RATIOS

Year	January	February	March	April	May	June	July
1904	117.05	101.62	95.91	92.93	87.68	88.16	84.63
1905	117.05	101.62	95.91	92.93	87.68	88.16	84.63
1906	117.27	104.93	95.56	92.60	87.36	87.84	84.33
1907	118.13	106.26	96.47	94.13	86.75	88.14	83.74
1908	122.35	106.96	96.80	93.83	86.18	88.05	83.18
1909	121.29	106.03	96.94	95.36	89.50	89.81	81.79
1910	121.40	106.50	99.91	95.44	89.58	86.87	81.86
1911	122.76	105.90	99.56	96.16	89.25	87.16	81.58
1912	122.74	108.18	99.23	96.24	89.12	87.47	81.86
1913	122.63	105.44	98.93	96.14	89.61	87.38	83.39
1914	121.68	104.96	99.52	96.04	90.40	86.97	85.03
1915	119.50	104.28	99.65	95.83	90.69	90.83	85.24
1916	118.38	103.71	99.57	97.58	92.32	91.59	85.40
1917	116.07	103.10	102.12	95.11	92.78	92.05	85.83
1918	118.48	102.66	101.69	97.66	92.39	91.66	85.47
1919	116.78	103.57	102.75	97.53	91.45	92.13	86.36
1920	117.79	104.43	101.55	98.37	90.28	86.80	84.72
1921	119.25	104.61	103.79	98.52	90.42	86.93	81.69
1922	117.89	103.42	106.77	100.42	91.33	87.42	80.76
1923	117.99	105.39	102.40	99.25	88.13	85.75	80.93

Year	August	September	October	November	December	Totals
1904	88.90	93.72	104.97	119.07	125.37	1200.00
1905	88.90	93.72	104.97	119.07	125.37	1200.00
1906	88.58	93.38	104.59	118.64	124.92	1200.00
1907	87.95	92.73	103.85	117.81	124.04	1200.00
1908	86.97	92.12	103.64	117.03	122.91	1200.00
1909	85.91	93.05	102.74	115.94	121.64	1200.00
1910	85.27	93.13	102.83	115.56	121.65	1200.00
1911	84.95	92.67	104.00	114.92	121.07	1200.00
1912	85.23	92.36	103.66	113.34	120.57	1200.00
1913	85.14	92.51	105.12	113.23	120.45	1200.00
1914	84.97	92.32	104.64	112.58	120.89	1200.00
1915	85.76	92.44	104.37	110.37	121.05	1200.00
1916	88.22	92.36	102.80	107.59	120.48	1200.00
1917	88.67	92.83	102.64	107.25	121.55	1200.00
1918	89.08	91.38	102.12	106.79	120.62	1200.00
1919	86.64	89.90	103.18	107.87	121.84	1200.00
1920	87.18	93.13	104.07	108.80	122.89	1200.00
1921	85.64	90.50	104.32	110.64	123.70	1200.00
1922	84.65	86.76	105.99	112.87	121.71	1200.00
1923	86.26	88.41	108.00	115.02	122.47	1200.00

THE LEAST SQUARES CRITERION FOR TREND LINES

By W. L. Crum

I. THE RELATIVE NATURE OF THE TREND CONCEPT

It is a familiar fact that the least squares method of fitting lines of secular trend to the time series of economic statistics gives a misleading impression of precision and finality. Those engaged in the analysis of such series are generally aware of the dangers involved in accepting without searching criticism the results afforded by the method, but little has been written upon this phase of the problem, and the rapidly expanding use of trend calculations in practical statistics suggests that an early warning may not be out of place.

It is well known that the method, although its application to a specific series extending over a specific interval yields a formal solution which is straightforward and unique, frequently leads to discordant results when applied to slightly different intervals in the same series. For example, if straight lines be fitted to the data of Table I, for the two intervals 1903–13 and 1904–14, the results are:

Interval	*Ordinate for 1908*	*Annual Increment*
1903–13	8983	245
1904–14	9055	83

The difference between the ordinates of trend for a particular year near the centre of both intervals is of course not great, but there is a striking difference between the estimated annual increments. This example is undoubtedly somewhat extreme, but cases of nearly as great divergence are by no means rare.

This emphasizes a point of view which is too seldom discussed. The application of most statistical methods to *time series* is in its essence a descriptive process, and the results are peculiarly subject to the limitations of the data actually used in the calculation. In the analysis of *frequency series* the statistical constants obtained are in many instances capable of extension beyond the series of data actually analyzed: for example, the average income for a properly selected group of individuals can be accepted as approximating the average for a much larger group from which the original group was chosen. This extension rests upon the theory of sampling, which is merely an application to statistical measurement of the theory of probability. Now, as Professor Persons

has indicated, the theory of probability does not apply in the same way to series ordered in time.[1] The statistical constants pertaining to a time series are essentially *relative:* relative to the data actually used. It is true that many such constants can be "extended" to cover differing or longer intervals of the original series, and some are much more capable of such extension than others. The fact remains, however, that the assumption, in the case of a time series, that the segment of the series actually analyzed can be regarded as a random sample of adjacent or longer segments is exceedingly dangerous. Nowhere is this danger greater than in the fitting of lines of trend.

Thus, in the illustration given, the two trends found are accurate statistical *descriptions*, each for its proper interval. If it be asked what was the long-time trend in New York Bank Clearings during the year 1910, the answer must be that no *absolute* numerical statement is possible. *Relative* to the interval 1903–13, it can be given; and likewise, *relative* to any other interval. Moreover, the inability to answer directly the question does not arise from inadequacies in our statistical methods: it is merely an acknowledgment that the question is incomplete until the term "long-time trend" is defined by the specification of an interval.

These observations are not intended to destroy confidence in the proper use of trend analyses. If it be borne in mind that the complete analysis of a given time series is merely a careful description of the particular segment analyzed, many dangers will be avoided. On such a basis, the various steps in the analysis are merely parts of the complete description. The determination of trend, of seasonal variation, of cycles, and of any other statistical characteristics of the series, serves merely to summarize in the readiest form the essential descriptive facts. The extension of this description beyond the interval studied implies a logical process which is generally non-statistical in its main aspects.

II. THE STATISTICAL FORM OF THE LEAST SQUARES OPERATION

If the view be accepted that any determination of trend is merely a step in statistical description, the question properly arises whether the description afforded by the least squares method is satisfactory. Scientific description is fundamentally classificatory in its nature, and a proper classification should be free from blur. The several influences operating to cause economic variation over time may be listed as trend, seasonal variation, cyclical fluctuation, and irregular disturbances. A perfectly satisfactory measurement of trend would be independent

[1] This JOURNAL, March, 1924.

of the presence in the series of variations due to the other influences. A measurement of trend which is considerably blurred by these other influences could not be accepted as highly satisfactory.

The all but automatic procedure involved in the least squares method tends to generate undue confidence in the sufficiency of the results. It is important to inquire in each case whether the method does satisfactorily describe the trend. The point is here, as in many other fields of applied statistics, that methods are not invariably applicable and that therefore proper critical examination of the solution of any particular problem involves adequate steps to test the validity and appropriateness of the methods used. It is the purpose of the following discussion to point out certain statistical properties of the least squares method and to suggest devices by which more complete knowledge may be gained of the actual significance of the least squares operation in any particular case. Tentative methods will also be outlined for distinguishing the cases for which the least squares method seems least satisfactory. The discussion will be confined to rectilinear trends, partly because of the greater facility with which appropriate illustrations can be found and treated, and partly because a very large portion of the problems arising in practice are of this sort.

The nature of the least squares criterion and the process of applying the method to the secular trend problem are quite familiar, but they will be summarized here for convenience of reference. According to this method the line of secular trend is a line located so that the sum of the squares of the deviations, measured normally to the time axis, of the several points from such line is a minimum.[1] Thus AB is the line of trend for the points represented by P_1, P_2, etc., in Figure 1, if AB is situated so that the sum of the squares of the vertical line-segments (for example, P_1P_1') is as small as possible. It is well known that a line fitted according to this principle is identical with the line of regression of the economic variable on the time, and that the process is equivalent to fitting by the method of moments.[2]

Table II gives an illustration of the direct application of the method to the data 1903–13 of Table I. Column 2 gives the values of the time t, measured from an origin at the center of the interval; and in Columns 3 and 4, respectively, appear the negative and positive "cross-moment" items yt. The position of the line of trend is fixed by the *central value*, 8983, which is the average of the y items, and the *annual increment*,

[1] The deviations can be measured in the direction other than that stated (see, for example, L. J. Reed's discussion of Pearson's suggested method of fitting a line, in *Metron* for April, 1921; and also H. S. Uhler's critical analysis in *Journal of the Optical Society of America and Review of Scientific Instruments* for November, 1923), but measurements perpendicular to the time axis are alone appropriate in ordinary time series of economic statistics.

[2] See, for example, W. M. Persons, *Review of Economic Statistics*, Prel. Vol. I, p. 12, footnote.

245, which is the ratio of the algabraic sum of the yt items to the sum of the squares of all values of t. The *equation* of secular trend is therefore:

$$y=245t+8983.$$

where t is measured in years from July 1, 1908, and y is measured in the same unit as the original data.

TABLE I

ANNUAL BANK CLEARINGS OF NEW YORK CITY *

(Unit: $10,000,000)

1903	6596	1909	10359
1904	6864	1910	9726
1905	9382	1911	9237
1906	10467	1912	10075
1907	8718	1913	9463
1908	7928	1914	8302

* Data from *Review of Economic Statistics*, Prel. Vol. I, p. 64. Unless otherwise stated, all data used in this article may be found in that volume, on pages to be learned from the index.

TABLE II

DIRECT CALCULATION OF TREND BY MOMENT METHOD

y (1)	t (2)	yt (3)	yt (4)
6596	−5	32980	.....
6864	−4	27456	.....
9382	−3	28146	.....
10467	−2	20934	.....
8718	−1	8718	.....
7928	0	.....	.....
10359	1	.....	10359
9726	2	.....	19452
9237	3	.....	27711
10075	4	.....	40300
9463	5	.....	47315
		−118234	+145137

$\Sigma y=98815$; $\Sigma t^2=110$; $\Sigma yt=26903$.
Central value=8983; annual increment=245.

TABLE III

ABRIDGED FORM OF TABLE II

y_{-t} (1)	y_t (2)	y_t-y_{-t} (3)	t (4)	$(y_t-y_{-t})t$ (5)
8718	10359	1641	1	1641
10467	9726	−741	2	−1482
9382	9237	−145	3	− 435
6864	10075	3211	4	12844
6596	9463	2867	5	14335
				+28820
				− 1917
				$\Sigma yt=26903$

Table III presents an abridged form of the above calculation. This is of interest because it affords a real economy in the application of the method and because it opens the way for the following theoretical discussion. The simplification leading to Table III rests upon the symmetrical form of the moment operation in Table II, which in turn is due in part to the central position of the time origin. The results obtained by the method of Table III are, of course, identical with those given above.

Each item of Column 5 of Table III has the form $(y_t-y_{-t})t$, which may be modified to:

$$\frac{y_t-y_{-t}}{2t}2t^2$$

Here the fraction is the mean annual increase over the period $-t$ to $+t$, as indicated by the two clearings items for those two years. In the 11-year interval studied, there are five such fractions. Obviously the *annual increment* may be regarded as a weighted average of these fractions, the weights being $2t^2$ in each case.[1]

It is from this point of view that the least squares method can well be examined. The method bases the position of the trend line on two averages: the central value is the simple average of the y items, the annual increment is the weighted average of the mean annual increases over the several time periods. It is the second of these averages which requires a more thorough examination.

If the number of years in the interval being studied is odd (the analysis can be extended to the case of an even number), and equal to n, there are $\frac{n-1}{2}$ time periods each beginning and ending symmetrically about the center of the interval; and hence there are $\frac{n-1}{2}$ ratios $\frac{y_t-y_{-t}}{2t}$, in general all different. Each such ratio is regarded as an observation on the desired annual increment; but, as all these observations are not considered equally accurate, weights are used in averaging the observations to secure the best available measure of the annual increment. Now the process used in averaging a series of observations and the precision of the result depend upon the form of the frequency distribution of the observations. It is therefore vital to inquire into the manner in which the observed ratios are distributed about their mean.

III. DISTRIBUTIONS OF THE RATIOS FOR ANNUAL SERIES

Division of the items of Column 3 of Table III by $2t$ yields:

t	1	2	3	4	5
$\frac{y_t-y_{-t}}{2t}$	821	−185	−24	401	287

and, if the items t (which will hereafter be referred to as the *periods* although strictly each t is only half a period) are ranked according to the size of the ratio, the order is:

2, 3, 5, 4, 1.

The same process was applied to twenty-three other series, for the interval 1903–13, and the results of the ranking of the periods are

[1]This has been shown by Edwin Frickey, *Review of Economic Statistics*, Prel. Vol. I, p. 210.

shown in Table IV. In the final column of the table are given the symbols designating the *type* of the cyclical fluctuations of each series, following in the main the grouping plan of Professor Persons. *Type A* is speculative, *B* is business, *C* is money; and the series having their symbols in parentheses move inversely to the type. In these inverse cases, the ranking order should be read from right to left rather than from left to right.

TABLE IV

DESIGNATION OF SERIES, AND RANKING OF THE PERIODS ACCORDING TO SIZE OF THE RATIO

Symbol	Series	Periods, arranged in order of increasing size of the "ratio"					Type
a	Bank clearings of New York City	2	3	5	4	1	*A*
b	Pig iron produced in the United States	1	3	2	5	4	*B*
c	Bank clearings outside New York City	1	2	3	5	4	*B*
d	Bradstreet's price index	1	3	5	2	4	*B*
e	Merchandise imports into the U. S.	1	3	2	5	4	*B*
f	Building permits of twenty leading cities	2	3	5	4	1	*A*
g	Gross earnings of ten leading railroads	1	2	5	3	4	*B*
h	Shares sold on the N. Y. Stock Exchange	2	3	5	4	1	*A*
j	Bradstreet's number of business failures	4	3	5	2	1	(*B*)
k	Yield on ten railroad bonds *	1	4 and 5		3	2	(*A*)
l	Rate on 4–6 months commercial paper	1	2	3	5	4	*C*
m	Rate on 60–90 day commercial paper *	1	2	3	5	4	*C*
n	Rate on call loans at the N. Y. S. E.	1	2	3	5	4	*C*
o	U. S. Bureau of Labor Statistics price index	1	5	3	2	4	*B*
p	Industrial dividend payments	1	3	2	5	4	*B*
q	Price of twelve industrial stocks	2	3	5	4	1	*A*
r	Price of twenty railroad stocks	2	3	5	4	1	*A*
s	Incorporations in Eastern States	2	5	3	1	4	*A*
t	Loans of N. Y. Clearing House banks	4	3	5	2	1	(*C*)
u	Deposits of N. Y. Clearing House banks	4	3	5	2	1	(*C*)
v	Reserves of N. Y. Clearing House banks	4	5	3	2	1	(*B*)
w	Net earnings of the U. S. Steel Corp.*	1	2	3	5	4	*B*
x	Dun's price index *	5	3	1	4	2	*B*
y	Bank clearings in Pittsburgh *	1	2	3	5	4	*B*

* Data for these series are given in Part II of *An introduction to the methods of economic statistics*, by W. L. Crum and A. C. Patton.

It will be observed that every series of *Type A* has the order of periods found above for Series *a*, except *s*. Among the series of *Type C* the similarity is almost as great, but it appears that Periods 3 and 5 have one order in the interest rate series and the opposite order in the banking series. For series of *Type B* the uniformity is by no means as good. Seven of the twelve cases fall under 1 3 2 5 4 or 1 2 3 5 4, which are very nearly identical; and two more are covered by 1 2 5 3 4, which is fairly close to the second but moderately different from the first of the above orders. The three remaining cases are the wholesale price series, which are all different and of which only Series *d* is reasonably close to any of the orders belonging to the nine more uniform cases.

The observed differences within each type can be explained completely by examining the time-curves of the individual series. It will be found that differences in the ranking order of series of any one type

arise from poor correlation of the cyclical movements of the particular series with the standard movements of that type. Ordinarily the timing of the fluctuations is not at fault, but there are differences in *shape* and *amplitude* of the cycles which account for inversions in the ranking order. It is the high uniformity, rather than the dissimilarities, of the ranking orders for series of each type, that is striking. Careful appraisal of the facts might have led us to foresee this uniformity, but the actual evidence given in Table IV is more forceful than any *a priori* reasoning.

The conclusion to be drawn from Table IV is that the ordering in rank of the periods, according to the size of the ratio $\frac{y_t - y_{-t}}{2t}$, is due to cyclical causes. The fact that this ordering is a cyclical phenomenon stamps it as systematic. If it were ever possible to entertain the thought that the distribution of ratios for any one series could be regarded as a random sample of a larger distribution, Table IV should dispel that thought. Chance variations in the size of individual ratios there undoubtedly are, but the dominant influence is not chance at all: it is the systematic working of the cyclical swings in the given series.

The order of rank in the array of ratios is, however, one of the least specific aspects of a frequency distribution. Table V exhibits the actual value of the ratio for each period for each series of Table IV, and gives also the computed annual increment in each case. The "frequency" distribution for any one of these series is of a specialized sort: the frequency of each observation is unity. Hence a diagram of the distribution for any series would convey little information; but some estimate of the precision of the result can be made by comparing, approximately, the dispersion with the average. It will be seen that the degree of concentration varies widely, from good in cases like *c*, *j*, *p*, and *t*, to very poor in cases like *a*, *m*, *s*, and *w*.

Even in the most favorable cases the concentration is much less marked than that ordinarily to be found in the fitting of rectilinear observational equations in problems of physical and natural science. The reason is obvious, the plotted points of an economic series are widely scattered from the line of trend. What the least squares process does in effect is to apply such weights to the several observations as to reconstruct a more nearly *normal* frequency distribution. The result departs from the usual form of graduated frequency series, for the individual frequencies are fixed and the variation is in the ratio alone.

It appears from Table IV that this artificial effect of the method is in some part corrective. Ratios for Period 1, which receive the lightest weight, are almost invariably at one extreme of an array. Moreover,

very few ratios of Period 2, which receives less than 8 per cent of the weight, fall at the center of an array. On the other hand, several ratios of Period 5 fall at or near the end of an array; and a large number of ratios of Period 4, which receives over 30 per cent of the weight, fall at one extreme of an array. A more satisfactory view can be obtained by plotting as abscissas, for each series, the ratios given in Table V and erecting at the plotted points ordinates representing t^2. The conclusion will only be strengthened that the distributions, even as "reconstructed" by the weighting process of the least squares method, do not in general conform even approximately to the normal curve. In almost every case the "curve" is strongly skewed, and in most cases a moderately large frequency is found dangling at one end. (See examples in Chart 2.)

TABLE V

DETAILS OF THE RATIO-DISTRIBUTIONS FOR SERIES OF TABLE IV

Symbol	Value of the ratio for period, t, equal to					Annual increment
	1	2	3	4	5	
a	821	−185	−24	401	287	245
b	45	515	123	1656	1329	1144
c	211	286	296	367	320	325
d	−.19	.14	.10	.16	.13	.12
e	263	606	589	978	797	792
f	545	−13	62	239	185	173
g	−77	253	379	417	373	370
h	911	−3004	−2266	−702	−777	−1130
j	799	551	452	427	478	470
k	−.11	.05	.04	.02	.02	.02
l	−1.00	−.13	−.07	.03	0.00	−.01
m	−1.20	−.17	−.06	.07	.01	−.02
n	−2.16	−.89	−.31	.22	−.05	−.11
o	−1.5	2.3	2.2	2.6	2.2	2.2
p	124	187	182	209	205	199
q	8.0	−2.3	.5	4.3	2.4	2.4
r	11.1	−3.5	−1.3	4.3	.5	.58
s	70	−91	24	148	−12	36
t	97	45	42	39	44	43
u	141	48	42	34	45	43
v	44	15	13	7	12	11
w	−14.7	−3.9	−2.6	4.4	2.8	1.6
x	3.0	3.4	2.7	3.0	2.2	2.6
y	−19.13	−1.34	.24	9.20	5.76	4.89

Units for the data are as follows: *a*, *c*, *y*, $10,000,000; *b*, 1,000 tons; *d*, *q*, *r*, *x*, $1; *e*, *f*, *g*, *p*, $100,000. *h*, 10,000 shares; *j*, 1 failure; *k*, *l*, *m*, *n*, *o*, 1 per cent; *s*, *t*, *u*, *v*, *w*, $1,000,000.

No doubt part of this irregularity arises from the fact noted above that the "frequencies" are fixed. A further cause is presumably the shortness of the interval and the consequently small number of ratios. Table VI shows the computations for a longer series, and Chart 3 is the corresponding "frequency" diagram. The concentration about the average is quite good, but here also the weighted series is skewed and carries a secondary "mode" at one end.

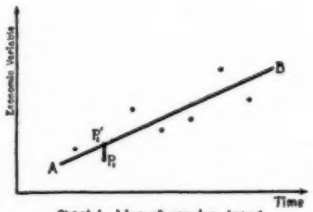


Chart 1. Line of secular trend

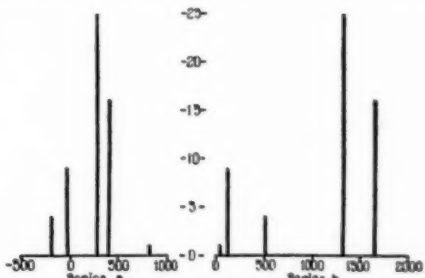


Chart 2. Types of ratio-distribution
(Ratio scale horizontal, "frequency" scale vertical)

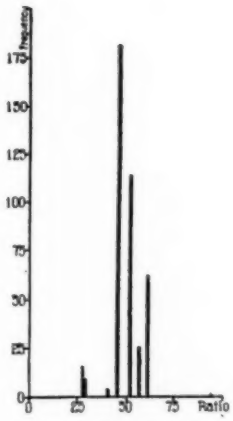


Chart 3. Ratio-distribution

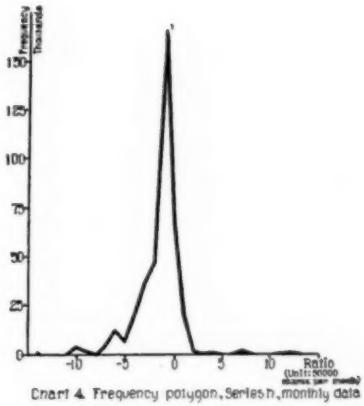


Chart 4. Frequency polygon, Series h, monthly data

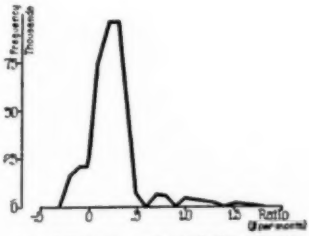


Chart 5. Frequency polygon, Series q, monthly data

It seems that, even after sufficient allowance is made on account of the inflexibility of the weighting system, the frequency distribution of the ratios is not such that the annual increment—which is one of the averages of that distribution—is highly precise. This conclusion holds even for a fairly long series, and only the more forcibly for the shorter series usually analyzed in practice.

TABLE VI

DEPOSITS IN NEW YORK CLEARING HOUSE BANKS

(Unit: $1,000,000)

t	y		$y_t - y_{-t}$	$(y_t - y_{-t})t$	$(y_t - y_{-t})/2t$
	(a)	(b)			
1	935	1122	187	187	94
2	958	1120	162	324	41
3	853	1024	171	513	29
4	844	1065	221	884	28
5	729	1297	568	2840	57
6	605	1347	742	4452	62
7	483	1216	733	5131	52
8	543	1372	829	6632	52
9	568	1395	827	7443	46
10	434	1344	910	9100	46

$\Sigma t^2 = 770$; $\Sigma yt = 37506$; Ann. incr. $= 48.7$

(a) Items for 1893–1902, read up. (b) Items for 1904–1913, read down. Item for 1903 is 899, which, with (a) and (b), yields the 21-year average.

IV. THE DISTRIBUTIONS FOR MONTHLY SERIES

In practice there is seldom any attempt to fit lines of trend to monthly data: the determination is almost invariably based upon annual averages or aggregates. In order, however, to secure a much larger number of "observations" and to be able to form an actual grouped frequency series, the method was applied for the interval January, 1903 to December, 1913, to two series, h and q. The resulting ratios—which are here the mean monthly increase for the several periods—were classified in groups of specified width, and the corresponding least squares "weights" were totalled to form the group frequencies.

The two frequency curves are shown in Charts 4 and 5. Each curve is strongly skewed, and each has a strikingly pronounced mode. Each departs widely from the Gaussian form. The chief effect of the weighting system appears to have been the sharp elevation of the mode; and a minor effect, which apparently can not be avoided, is the stressing of some of the extreme variations.

Consideration of these charts shows that the arithmetic mean—the *annual increment* as computed—of such series is not highly typical. It might be suggested that, for these particular series, the mode would be a more satisfactory value for the annual increment. Or, if it be urged that the mode is too subject to displacement because of the peculiar weighting system, the choice might fall upon the median, which is almost certainly between the mean and the mode in cases such as these. But these speculations are largely idle, since neither the mode nor the median seems available for annual series such as shown in Chart 2.

In fact, Charts 4 and 5 teach us little which can be carried over to the practical problem of annual series. These monthly distributions add merely to our knowledge of the manner in which the peculiar weighting scheme of the least squares method affects the frequencies. They serve, with the observed fact that the form of the frequency distribution of ratios is fundamentally cyclical, to emphasize further the dangers in a blind use of the method.

It should be remarked that there are some exceptionally large ratios in these monthly illustrations. This is to be expected: monthly figures are much more subject to irregular disturbances than are annual data. The irregular disturbance in an economic series seldom persists more than one or two months, and is therefore nearly obliterated in the annual average or aggregate. Hence there is stronger reason for "rejecting" entirely certain ratios in the monthly problem than in the annual. Thus, in computing the annual increment from monthly data, those items $y_t - y_{-t}$ for which the ratio falls extremely far from the mean might be disregarded. This process may, however, lead to a worse evil than it seeks to correct; for, if there is seasonal variation in the given series, the omission of certain pairs of months in calculating the annual increment may cause the result to depend in part on the seasonal influence. It can be shown that, if no pairs of months are omitted, the least squares result is independent, or nearly so, of any seasonal variation present in the series. The omission of some months may set aside this relation.

The other series of Table IV have not been examined in monthly form. There is no reason to expect that the resulting distributions would in general closely resemble those of Charts 4 and 5, nor, on the other hand, that they would have the normal form. It is probable that each would differ markedly from the normal type, and that peculiarities in the curve would be traceable largely to the least squares weights and to the basic cyclical movements. Although the additional information, bearing on any one series, obtainable from the analysis of the monthly data may well be quite extensive, it was not thought that the analysis of all these monthly series would contribute sufficiently to the general problem to justify the labor of computation.

V. SUMMARY AND CONCLUSIONS

The foregoing discussion has sought to emphasize the inadequacy of the least squares method for determining lines of trend. It was noted that the fitting of a line of trend is a descriptive process, and that it is relative to the specific interval studied. Even as a description of the tendency over a particular interval, the least squares result was ob-

served to lack seriously in precision. The wide dispersion of the ratios about their mean, and the dominating influence of the cyclical movement on the distribution of ratios prevent the acceptance of the annual increment as merely an average of independent observations subject to chance errors. Examination of the least squares method as a process of weighting these observations indicates that the method tends to reduce in part the effects of the extreme dispersion among the ratios, but it brings out also that much of the force of this inflexible weighting schedule may at times fall near the extreme of the distribution.

The farther the inquiry is carried the more clearly does it appear that the method is not automatically safeguarded. In a number of the specific instances studied, the process seems to yield results which are satisfactorily precise. In other cases it falls far short. No general rule stands forth: it seems necessary to examine each case with care.

It is suggested that an additional step may well be added to the usual computation of trend: the frequency diagram (such as Chart 3) should be formed and carefully examined. This diagram may indicate that a change in the interval is desirable; or, in case of a long series, that certain ratios should be rejected from the computation. It may even lead to the use of some mean other than the arithmetic, or to a weighting scheme differing from that of least squares. It should be remembered, however, that all these changes—particularly those touching the method of computing the annual increment—must be made in the realization that they alter somewhat the accepted standard determination of trend. In problems in which comparison between series is the ultimate goal, this consideration may well be controlling. In fact this is one of the chief reasons for accepting in general the least squares method with all its faults, but such a view can well be given too prominent a place.

Even if the frequency diagram leads to no specific modification of the computation, it will certainly give the investigator a clearer understanding of the case than he would otherwise get. For this reason it may be well in some instances to devote additional labor to the formation of the frequency curve for the monthly data. A thorough knowledge of the elements from which the annual increment is constructed should be one of the aims of all these supplementary inquiries.

A full realization of the inevitable limitations of certain statistical methods as applied to the analysis of time series, and a healthy fear of putting undue faith in simple processes which do not admit of thorough theoretical justification—these will help the statistical worker to firmer footing.

CAMBRIDGE, MASS.,
3 September, 1924.

THE LAW OF DEMAND AND THE THEORY OF PROBABILITY

By Theodore H. Brown

The purpose of the present paper is to show that there is a logical basis for a certain group of curves which may be fitted to economic data. At present many of the curves used by statisticians have been borrowed from other fields of knowledge or built up empirically. This procedure may do well enough during the experimental development of a subject, but statistics has reached a point where, instead of borrowing curves and answers from other scientific fields, the logic or methods of reasoning used in discovering answers should be borrowed.

The plan of the present paper is to borrow mathematical reasoning from the field of life insurance as it is used to develop a mathematical expression for the law of mortality. A number of such expressions are known which have been summarized in the work of M. Albert Quiquet.[1] This work of Quiquet shows that certain mortality laws such as that of Makeham are only special cases of a more general law. Since Quiquet's application is to life insurance, his approach does not put the reasoning in such a form that it is readily applicable to the problems of economics. We shall recast, therefore, the language of his theorem for our present purpose.

Suppose that the two variables x and y are connected by the relation

$$y_i = l_i(x_i) \qquad i = 1, 2, 3, \ldots n$$

where the $l_i(x_i)$ may or may not be all different and where the subscript i for the points (x_i, y_i) are used to indicate that the origin may or may not be different for each $l_i(x_i)$.

Assume that the function

$$y_i = l_i(x_i)$$

gives the number of times an event occurs for a given value of x_1.

Suppose that if we have

$$x_1 = a,$$
$$x_2 = b,$$
$$\vdots$$
$$x_n = k.$$

[1] Quiquet: "Representation Algebrique des Tables de Survie"; *Bulletin de L'Institut des Actuaires Français*, No. 14, 1893.
"Sur L'Emploi Simultane de Lois de Survie Distinctes"; *Fourth International Congress of Actuaries.*

Then $l_1(a), l_2(b), l_3(c), \ldots l_n(k)$ will give the number of times certain events will occur under various laws for corresponding values of x_i. Similarly $l_1(a+t), l_2(b+t), \ldots$ will give the corresponding number of times certain events will occur when $x_1=a+t$, $x_2=b+t, \ldots$ Then if out of $l_1(a), l_2(b), \ldots$ possible events we have $l_1(a+t), l_2(b+t), \ldots$ events occurring, the probability that some one event of the $l_1(a)$ events will be included in the $l_1(a+t)$ events is $\frac{l_1(a+t)}{l_1(a)}$. We can obtain a similar result for each of the $l_i(x_i)$. The probability that one of each of the original events will all be included in the final events is the product of the separate probabilities.[1] Thus we have

$$\frac{l_1(a+t)}{l_1(a)} \cdot \frac{l_2(b+t)}{l_2(b)} \cdot \frac{l_3(c+t)}{l_3(c)} \cdots \frac{l_n(k+t)}{l_n(k)} = G(\alpha, \beta, \gamma \ldots \theta, t) \quad (1)$$

where $\alpha, \beta, \gamma \ldots \theta$ represent the n functions of the x_i.

If the respective forms of the functions $l_i(x_i)$ can be determined for all values of x, we will have the law of variation of y expressed as a function of x.

Take the logarithm[2] of equation (1). This gives

$$\log[l_1(a+t)]+\log[l_2(b+t)]+\log[l_3(c+t)]+ \ldots$$
$$-\log[l_1(a)]-\log[l_2(b)]-\log[l_3(c)]- \ldots = \log G(\alpha, \beta, \gamma \ldots \theta, t).$$

If we differentiate this with respect to t, we have

$$\frac{l'_1(a+t)}{l_1(a+t)}+\frac{l'_2(b+t)}{l_2(b+t)}+\frac{l'_3(c+t)}{l_3(c+t)}+ \ldots +\frac{l'_n(k+t)}{l_n(k+t)}=\frac{G'(\alpha, \beta, \gamma \ldots \theta, t)}{G(\alpha, \beta, \gamma \ldots \theta, t)}$$

Setting

$$\frac{l'_1(a+t)}{l_1(a+t)}=\mu_1; \quad \frac{l'_2(b+t)}{l_2(b+t)}=\mu_2; \ \ldots \ ; \quad \frac{l'_n(k+t)}{l_n(k+t)}=\mu_n$$

and $$\frac{G'(\alpha, \beta, \gamma \ldots \theta, t)}{G(\alpha, \beta, \gamma \ldots \theta, t)}=F(\alpha, \beta, \gamma \ldots \theta, t)$$

we have

$$\mu_1(a+t)+\mu_2(b+t)+ \ldots \mu_n(k+t)=F(\alpha, \beta, \gamma \ldots \theta, t). \quad (2)$$

As equation (2) is not only continuous but its derivatives are also continuous, take the first n derivatives of (2) and set $t=0$. This will give us the system

[1] If we want the probability that two or more events shall all (jointly) occur, we multiply the respective probabilities of their individual occurrence. This probability is sometimes called the "and also" (multiplication) probability in contrast to the "either or" (addition) probability which gives the probability that at least one of several mutually exclusive events will happen.

[2] Throughout the paper logarithms are to the base e.

$$\begin{aligned}
\mu_1(a)+\mu_2(b)+ \ . \ . \ . \ +\mu_n(k)&=F_0\\
\mu'_1(a)+\mu'_2(b)+ \ . \ . \ . \ +\mu'_n(k)&=F_1\\
&\vdots\\
\mu^n_1(a)+\mu^n_2(b)+ \ . \ . \ . \ +\mu^n_n(k)&=F_n
\end{aligned} \tag{3}$$

where F_0, F_1, F_2, . . . F_n are $F(\alpha, \beta, \gamma \ . \ . \ . \ \theta, t)$ and its derivatives when $t=0$.

Quiquet now says:

Since the second members of (3) are $n+1$ in number, but are functions of only n variables, it follows that there exists some relation between them and hence also between the first members. These first members consist of $n+1$ functions of n independent variables a, b, c, . . . k. The necessary and sufficient condition that there be a relation between the first members independent of the variables is that the functional determinant of these $n+1$ functions with respect to any $n+1$ of the variables shall be equal to zero. One of these determinants is

$$\begin{vmatrix}
\mu'_1(a), & \mu'_2(b) & . \ . \ . & \mu'_n(k)\\
\mu''_1(a), & \mu''_2(b) & . \ . \ . & \mu''_n(k)\\
& \vdots & & \\
\mu^{n+}_1(a), & \mu^{n+1}_2(b) & . \ . \ . & \mu^{n+1}_n(k)
\end{vmatrix} \tag{4}$$

In order that this determinant shall be identically zero, it is necessary and sufficient that there be similar relation linear and homogeneous between all the elements of each of its lines or each of its columns.

Taking the first column one of these relations is

$$A_0\mu'_1(a)+A_1\mu''_1(a)+ \ . \ . \ . \ +A_n\mu_1{}^{n+1}(a)=0 \tag{5}$$

where A_0, A_1, A_2, . . . A_n are independent of a and not all zero.

Since the value of a is arbitrary, we may replace it in (5) by x. This gives

$$A_0\mu'_1(x)+A_1\mu''_1(x)+ \ . \ . \ . \ +A_n\mu^{n+1}(x)=0. \tag{6}$$

Similar equations can be obtained from other columns of the determinant for each of the other laws of change.

If we set

$$\mu'(x)=z$$

in (6) we obtain a linear differential equation with constant coefficients. This is

$$A_nz^n+A_{n-1}z^{n-1}+A_{n-2}z^{n-2}+ \ . \ . \ . \ +A_2z''+A_1z'+A_0z=0. \tag{7}$$

The solution of this equation is found by assuming

$$z=e^{rx}.$$

Differentiating, substituting the values of the derivatives in (7) and dividing through by e^{rx} gives the auxiliary equation

$$A_n r^n + A_{n-1} r^{n-1} + A_{n-2} r^{n-2} + \ldots + A_2 r^2 + A_1 r + A = 0. \tag{8}$$

This is an equation of the nth degree from which we find the values of r. These values fall into four classes. They may be real and all different. They may be real with some of them equal. They may be conjugate imaginary pairs and all different. They may be conjugate imaginary pairs with some of them equal. A single solution may contain terms of each of the various classes.

If the roots are all different, the solution of the equation is

$$z = C_0 e^{r_0 x} + C_1 e^{r_1 x} + \ldots + C_n e^{r_n x}. \tag{9}$$

If j of the roots are real and equal, the solution for those j roots is

$$z = C_0 e^{r_1 x} + C_1 x e^{r_1 x} + C_2 x^2 e^{r_1 x} + \ldots + C_{j-1} x^{j-1} e^{r_1 x}.$$

Factoring the $e^{r_1 x}$ we have

$$z = e^{r_1 x}(C_0 + C_1 x + C_2 x^2 + \ldots + C_{j-1} x^{j-1})$$

or

$$z = e^{r_1 x} \phi(x) \tag{10}$$

where $\phi(x)$ stands for the polynomial.

When there is an imaginary pair of roots, that part of the complete solution is of the form

$$z = e^{ax}[C_1 \operatorname{Sin} bx + C_2 \operatorname{Cos} bx]. \tag{11}$$

When the imaginary roots are repeated, that is when there are two or more equal pairs, the solution as in the case of (10) will have powers of x in the coefficient with the arbitrary constants. The solution in this case is

$$z = e^{ax}[P(x) \operatorname{Sin} bx + Q(x) \operatorname{Cos} bx] \tag{12}$$

where $P(x)$ and $Q(x)$ are polynomials in x, the degree being determined by the number of times the roots are repeated.

Since

$$\int z\,dx = \int u'(x)\,dx = \frac{l'(x)}{l(x)} = \frac{y'}{y}$$

and

$$\int \frac{l'(x)}{l(x)}\,dx = \log [l(x)] = \log y$$

integrating equations (9) to (12) twice will give various expressions for $\log y$.

As the integration is termwise and as the terms are only of a few

types, we can get the form of the final equations by considering the integration of these types. In general the integration gives terms of the same kind but with complicated coefficients made up of the original arbitrary constants. A single letter is substituted in place of the complicated coefficient. Since there are two integrations to be performed, two new arbitrary constants will be introduced. Consequently there will appear in each final expression the terms $A+Bx$.

In (9) the integration is performed only on terms of the type Ce^{rx}. Integrating such a type gives another of the same type but with a different arbitrary constant for a coefficient. As the value of the coefficient is determined by the data when the curve is fitted, a single letter can be used. Integrating (9), therefore, gives

$$\log y = A+Bx+Ce^{rx}+De^{rx} \ . \ . \ . \qquad (13)$$

In (10) if $r=0$, we integrate only powers of x. Keeping in mind the convention adopted above in regard to the arbitrary constants, we have on integrating

$$\log y = A+Bx+Cx^2+Dx^3+ \ . \ . \ . \qquad (14)$$

In (10) if $r\neq 0$, we have to integrate terms of the form $x^n e^{rx}$. The integration of terms of this form gives others of the same form, the number of terms depending on the power of x. Thus

$$\int C_1 x^2 e^{rx} dx = \left(\frac{C_1}{r}\right)x^2 e^{rx}+\left(\frac{-2C_1}{r^2}\right)xe^{rx}+\left(\frac{2C_1}{r^3}\right)e^{rx}+C_2.$$

When several terms of this form are integrated, we combine resulting like terms. Using a single arbitrary constant for each term, we get

$$\log y = A+Bx+Ce^{rx}+Dxe^{rx}+ \ . \ . \ . \qquad (15)$$

Excepting the constants of integration, integration of (11) gives an equation of a similar form. This is

$$\log y = A+Bx+e^{ax}[C \text{ Sin } bx+D \text{ Cos } bx]. \qquad (16)$$

Just as in the case of the repeated real roots, the integration of (12) gives another expression of the same form so that we have

$$\log y = A+Bx+e^{ax}[R(x) \text{ Sin } bx+S(x) \text{ Cos } bx]. \qquad (17)$$

Here the $R(x)$ and $S(x)$ are polynomials with arbitrary coefficients. The degree of $R(x)$ and $S(x)$ is equal to the number of times the roots are repeated.

Although equations (13) to (17) are given as separate solutions, it should be remembered that the complete solution of the differential equation is the sum of these separate solutions with the exception of (14) which is a special case.

APPLICATION TO DEMAND CURVES

The law of demand may be defined as follows: If at a particular instant for a certain price a certain number of units of a commodity can be sold, then for a greater price a smaller number of those units can be sold. The law states that as the price is increased the number of units which will be taken will be correspondingly smaller. If we sketch this condition on a set of coördinate axes so that the units are measured off along the y-axis and the price along the x-axis, it will be seen that the curve slopes downward in the direction of the positive x-axis.

This law can be stated in terms of the theory of probability, if we add that it is supposed that each of the units offered are uniformly acceptable—a thing which is usually tacitly assumed. We can say that the probability that a certain unit of the group offered at a small price will also be in the group sold at a larger price is the ratio of the number demanded at the larger price to the number demanded at the smaller price. In other words, the probability that a particular unit is sold at a larger price is the ratio of the number of successful choices (the smaller number of units demanded at a larger price) to the number of equally possible choices (the larger number demanded at the smaller price).

Thus for a uniform group of units of a commodity, if $l_1(a)$ are demanded at a price a and $l_1(a+t)$ at a price $a+t$, the probability that some particular unit of the $l_1(a)$ units will be also in the $l_1(a+t)$ units is

$$\frac{l_1(a+t)}{l_1(a)}.$$

If we want the probability that two particular units of the $l_1(a)$ units are both selected in the $l_1(a+t)$ units, we have by the theorem of compound probability

$$\frac{l_1(a+t)}{l_1(a)} \cdot \frac{l_1(a+t)}{l_1(a)}.$$

Furthermore, it makes no difference whether we have other groups of units of the same commodity following a different law of demand or whether we have units of other commodities following their own law of demand. We may have one group of units of one commodity following one demand law $l_1(x_1)$, another group of units of the same or a different commodity following a second demand law $l_2(x_2)$, etc. If, in particular, we have $l_1(a)$ units of one group, $l_2(b)$ of another . . . demanded at prices a, b . . . and $l_1(a+t)$, $l_2(b+t)$ . . . demanded at prices $a+t$, $b+t$, . . . then the probability that some particular unit

of each of these groups offered at prices a, b, . . . will be demanded jointly at prices $a+t$, $b+t$, . . . is

$$\frac{l_1(a+t)}{l_1(a)} \cdot \frac{l_2(b+t)}{l_2(b)} \cdot \frac{l_3(c+t)}{l_3(c)} \cdots \tag{18}$$

In other words, it makes no difference what the commodities are as the expression only gives the compound probability that a particular unit of each group offered at one price will be demanded at another.

The generality of the mathematical statement is interesting. It places no restrictions on buyers, prices, quantities of units or commodities. They may be all the same or all different. We can have pineapples, pig iron, garlic and kid gloves each represented in the same probability statement by its own law of demand.

We now ask the form of the curves $l_1(x_1)$, $l_2(x_2)$. . . As the expression (18) is the same as the left-hand side of (1), we have equations (13) to (17) or any sum of them as equations of demand curves.

In stating the law of demand it was assumed that the various units of the commodity offered were of uniform quality. If this were not so, the particular units taken at a higher price would be *selected* and not picked out by random chance. Failure to fit satisfactory demand curves to data may arise from this very lack of uniformity in the quality of the units offered at various prices. This produces a scatter which may make it very difficult to fit a single demand curve for a particular commodity. As an example, consider the demand curves for shipments into a given market of a perishable fruit like watermelons. Differences in size, quality of flavor and ripeness produce a scatter which make it difficult if not impossible to fit a curve, which means anything, to all shipments. It must be recognized, however, that it may be absolutely impossible to secure figures for various groups or shipments of a commodity. But whenever possible various groups of even a single commodity should be separated on the basis of uniform quality.

We will now show that the demand curves given by Professor H. L. Moore[1] are closely allied with one of the forms given here.

Equation (14) is, taking only three terms,

$$\log y = A + Bx + Cx^2.$$

Taking the derivative and then multiplying by x we get

$$\frac{x}{y}\frac{dy}{dx} = Bx + 2Cx^2. \tag{19}$$

[1] This JOURNAL, March, 1922.

Professor Moore shows in his paper that $\frac{x}{y}\frac{dy}{dx}$ is equal to the flexibility of prices. He assumes that this is equal to

$$\alpha+\beta x+\gamma x^2.$$

This expression and the right-hand side of equation (19) are equal if

$$\alpha=0$$
$$\beta=B$$
$$\gamma=2C.$$

Thus if $\alpha=0$ the demand curves developed by Professor Moore are included in the larger group of demand curves developed here.

In another paper I shall continue the discussion with special reference to trend and growth curves.

NOTES

THE REFINEMENT OF TIME SERIES

By Olin Ingraham

My objection to what may be called the standard methods of the refinement of time series is that these methods do not fit best to economic ideas and needs. The economist wants a base line from which to measure deviations and predict the least improbable future. He wants a unit of deviation first for graphic uses and secondly as a unit for mathematical comparisons. He wants mathematical comparisons to obtain information of similarity and lag in regard to peaks and troughs, increase and decrease, and average differences of position in units of deviation. And he is given the line of least squares, the standard deviation, and the coefficient of correlation, which whatever formal truths they tell him do not give him the information he needs.

And first the secular trend. The line of least squares seems to an economist a line of least deviations weighted by their own quantity. To use as a base line from which to measure deviations a line determined for weighted deviations seems curious. Nor does the formula of the slope $\frac{\Sigma xy}{\Sigma x^2}$ bring any reassurance. That the slope should be determined by weighting the original items by the length of time from the time center is a form of weighting which has no economic meaning. Besides, it is a form of weighting which leads to absurdly unstable results. We are dealing not with hit or miss facts, but facts in sequence. And the sequence of these facts in terms of a quadrilateral field can be most generally called a straight line zigzag. Of course this is a generalization. Each series may have its own particular form. But if we combine together five unlike series—I have chosen New York Bank Clearings, Pig Iron Production, Bradstreet's Prices, Imports and Building Permits—and use the method of change from month to month relative to the corrected position in deviations of the original month, the average change from month to month where the original month was between $+.5$ and $-.5$ was only 8 per cent less than the average of months that had a greater deviation from the secular trend. Dealing with a period which includes only three or four main zigzags we are at the mercy of the positions on the zigzag which the time extremities occupy.

One test of this instability is the amount of change in the secular trend which a slight change in the group of years included produces. Starting with the slopes given in Persons' *Indices of General Business Conditions*, I found the slopes for the somewhat smaller number of included years. I used the same five unlike series. For four of them Persons' secular trend covers the years 1903–1916. For those four I found the secular trend for 1903–1913, 1904–1914, 1905–1915 and 1906–1916. For one series—Bradstreet—Persons' secular trend covered the years 1903–1914. In that case I used included nine-year periods.

The following are the results:

New York Bank Clearings—Persons' +295; My included periods +244, +82, +47, and +337.

Pig Iron Production—Persons' +95; My included periods +122, +101, +69 and +114.

Bradstreet's prices—Persons' +110; My included periods +115 +130, +123 and +92.

Imports—Persons' +70; My included periods +79, +76, +68 and +88.

Building Permits—Persons' +99; My included periods +178, +65, −11, and +25.

The secular trends of none of these series are very stable. In two of these series they are in wild conflict among themselves. Now the chief reason for this lack of stability is simply that the waves of the original series are not smoothly repetitious. But the instability is increased by this peculiar slope formula as a study of the charts on page 200 of Persons' *Indices of General Business Conditions* will make evident.

Against this conception of a line of least squares we might embody as far as possible a line of arithmetic means. The line of least squares passes through the coördinated means of the series as a whole, but, barring accidents, points on one half of the line will have an arithmetic average higher or lower than the arithmetic average of the corresponding points of the original data. And for the other half of the line, of course, the situation will be reversed. Thus this secular trend which is used as a base line for measuring the plus or minus deviations of the original data, and for indicating the least improbable future, in the arithmetic average sense slopes itself in relation to the original data. And so I would urge in its place, what has been urged before, that the secular trend be the line that passes through the coördinated means of the two time halves of the series (and thereby, of course, through the coördinated means of the time series as a whole). Points on the line of coördinated means for the two time halves have the same arithmetic

average as the corresponding points of the original data. The line can be used for predicting the future by showing the arithmetic, or if we use logarithms, geometric progress between the two time halves of the series.

Yet in using this line of coördinated means as the secular trend, certain questions arise. In the first place, I was surprised at the smallness of the difference of two secular trends calculated with such a different system of weights. Pairing the two results in the five series we have as follows: +295 and +276, +95 and +112, +110 and +120, +70 and +84, and +99 and +92. In only two of the series is the difference greater than 10 per cent. I have compared the two methods in other series, and it is rare that the difference is very great. In the second place, the secular trend of coördinated means shows almost as great instability, as tested above, as the line of least squares, although there is no progressive weighting towards the time extremities; on the other hand, the very equality of the weighting gives arbitrary results with the shifting of the time center. For a peak or depression will be swung now this side, now the other side of the time center and thus add in a fortuitous way to the average of a time half. Now such peaks or depressions near the time center would have a minimum effect on the line of least squares.

Thus the line of coördinated means solves the problem of stability little more than the line of least squares, if calculated from the original data. But if calculated from a three-year moving average, we get, of course, the stability of the mean of three secular trends of slightly shifted years. And using this moving average, don't we get a secular trend that more exactly suits our demand? We want a line that will not be disturbed too much by the quantities of the extreme years. These may represent either the accident of the particular phase of the business cycle with which the series begins and ends, or a change in the real secular trend. Both of these reasons justify a reduction of weights for the extreme years. The moving average gives them one third and two thirds the weight of interior years. Again the use of the moving average blends over both time halves the phase of the business cycle nearest the time center. Also for those who prefer the line of least squares, a moving average—preferably of five or seven years—would take a good deal of the nonsense out of its slope formula. Of course the use of the moving average qualifies one of the claims for the use of the line of coördinated means. The averages of the ordinates of its two time halves would no longer be identical with the averages of the unweighted original data. But for such difference as would exist there would seem to be sufficient reason.

APPLICATION TO DEMAND CURVES

The law of demand may be defined as follows: If at a particular instant for a certain price a certain number of units of a commodity can be sold, then for a greater price a smaller number of those units can be sold. The law states that as the price is increased the number of units which will be taken will be correspondingly smaller. If we sketch this condition on a set of coördinate axes so that the units are measured off along the y-axis and the price along the x-axis, it will be seen that the curve slopes downward in the direction of the positive x-axis.

This law can be stated in terms of the theory of probability, if we add that it is supposed that each of the units offered are uniformly acceptable—a thing which is usually tacitly assumed. We can say that the probability that a certain unit of the group offered at a small price will also be in the group sold at a larger price is the ratio of the number demanded at the larger price to the number demanded at the smaller price. In other words, the probability that a particular unit is sold at a larger price is the ratio of the number of successful choices (the smaller number of units demanded at a larger price) to the number of equally possible choices (the larger number demanded at the smaller price).

Thus for a uniform group of units of a commodity, if $l_1(a)$ are demanded at a price a and $l_1(a+t)$ at a price $a+t$, the probability that some particular unit of the $l_1(a)$ units will be also in the $l_1(a+t)$ units is

$$\frac{l_1(a+t)}{l_1(a)}.$$

If we want the probability that two particular units of the $l_1(a)$ units are both selected in the $l_1(a+t)$ units, we have by the theorem of compound probability

$$\frac{l_1(a+t)}{l_1(a)} \cdot \frac{l_1(a+t)}{l_1(a)}.$$

Furthermore, it makes no difference whether we have other groups of units of the same commodity following a different law of demand or whether we have units of other commodities following their own law of demand. We may have one group of units of one commodity following one demand law $l_1(x_1)$, another group of units of the same or a different commodity following a second demand law $l_2(x_2)$, etc. If, in particular, we have $l_1(a)$ units of one group, $l_2(b)$ of another . . . demanded at prices a, b . . . and $l_1(a+t)$, $l_2(b+t)$ . . . demanded at prices $a+t$, $b+t$, . . . then the probability that some particular unit

of each of these groups offered at prices $a, b, \ldots$ will be demanded jointly at prices $a+t, b+t, \ldots$ is

$$\frac{l_1(a+t)}{l_1(a)} \cdot \frac{l_2(b+t)}{l_2(b)} \cdot \frac{l_3(c+t)}{l_3(c)} \cdots \tag{18}$$

In other words, it makes no difference what the commodities are as the expression only gives the compound probability that a particular unit of each group offered at one price will be demanded at another.

The generality of the mathematical statement is interesting. It places no restrictions on buyers, prices, quantities of units or commodities. They may be all the same or all different. We can have pineapples, pig iron, garlic and kid gloves each represented in the same probability statement by its own law of demand.

We now ask the form of the curves $l_1(x_1)$, $l_2(x_2)$. . . As the expression (18) is the same as the left-hand side of (1), we have equations (13) to (17) or any sum of them as equations of demand curves.

In stating the law of demand it was assumed that the various units of the commodity offered were of uniform quality. If this were not so, the particular units taken at a higher price would be *selected* and not picked out by random chance. Failure to fit satisfactory demand curves to data may arise from this very lack of uniformity in the quality of the units offered at various prices. This produces a scatter which may make it very difficult to fit a single demand curve for a particular commodity. As an example, consider the demand curves for shipments into a given market of a perishable fruit like watermelons. Differences in size, quality of flavor and ripeness produce a scatter which make it difficult if not impossible to fit a curve, which means anything, to all shipments. It must be recognized, however, that it may be absolutely impossible to secure figures for various groups or shipments of a commodity. But whenever possible various groups of even a single commodity should be separated on the basis of uniform quality.

We will now show that the demand curves given by Professor H. L. Moore[1] are closely allied with one of the forms given here.

Equation (14) is, taking only three terms,

$$\log y = A + Bx + Cx^2.$$

Taking the derivative and then multiplying by x we get

$$\frac{x}{y}\frac{dy}{dx} = Bx + 2Cx^2. \tag{19}$$

[1] This JOURNAL, March, 1922.

Professor Moore shows in his paper that $\frac{x}{y}\frac{dy}{dx}$ is equal to the flexibility of prices. He assumes that this is equal to

$$\alpha+\beta x+\gamma x^2.$$

This expression and the right-hand side of equation (19) are equal if

$$\alpha=0$$
$$\beta=B$$
$$\gamma=2C.$$

Thus if $\alpha=0$ the demand curves developed by Professor Moore are included in the larger group of demand curves developed here.

In another paper I shall continue the discussion with special reference to trend and growth curves.

NOTES

THE REFINEMENT OF TIME SERIES

By Olin Ingraham

My objection to what may be called the standard methods of the refinement of time series is that these methods do not fit best to economic ideas and needs. The economist wants a base line from which to measure deviations and predict the least improbable future. He wants a unit of deviation first for graphic uses and secondly as a unit for mathematical comparisons. He wants mathematical comparisons to obtain information of similarity and lag in regard to peaks and troughs, increase and decrease, and average differences of position in units of deviation. And he is given the line of least squares, the standard deviation, and the coefficient of correlation, which whatever formal truths they tell him do not give him the information he needs.

And first the secular trend. The line of least squares seems to an economist a line of least deviations weighted by their own quantity. To use as a base line from which to measure deviations a line determined for weighted deviations seems curious. Nor does the formula of the slope $\frac{\Sigma xy}{\Sigma x^2}$ bring any reassurance. That the slope should be determined by weighting the original items by the length of time from the time center is a form of weighting which has no economic meaning. Besides, it is a form of weighting which leads to absurdly unstable results. We are dealing not with hit or miss facts, but facts in sequence. And the sequence of these facts in terms of a quadrilateral field can be most generally called a straight line zigzag. Of course this is a generalization. Each series may have its own particular form. But if we combine together five unlike series—I have chosen New York Bank Clearings, Pig Iron Production, Bradstreet's Prices, Imports and Building Permits—and use the method of change from month to month relative to the corrected position in deviations of the original month, the average change from month to month where the original month was between $+.5$ and $-.5$ was only 8 per cent less than the average of months that had a greater deviation from the secular trend. Dealing with a period which includes only three or four main zigzags we are at the mercy of the positions on the zigzag which the time extremities occupy.

Professor Moore shows in his paper that $\frac{x}{y}\frac{dy}{dx}$ is equal to the flexibility of prices. He assumes that this is equal to

$$\alpha+\beta x+\gamma x^2.$$

This expression and the right-hand side of equation (19) are equal if

$$\alpha=0$$
$$\beta=B$$
$$\gamma=2C.$$

Thus if $\alpha=0$ the demand curves developed by Professor Moore are included in the larger group of demand curves developed here.

In another paper I shall continue the discussion with special reference to trend and growth curves.

NOTES

THE REFINEMENT OF TIME SERIES

By Olin Ingraham

My objection to what may be called the standard methods of the refinement of time series is that these methods do not fit best to economic ideas and needs. The economist wants a base line from which to measure deviations and predict the least improbable future. He wants a unit of deviation first for graphic uses and secondly as a unit for mathematical comparisons. He wants mathematical comparisons to obtain information of similarity and lag in regard to peaks and troughs, increase and decrease, and average differences of position in units of deviation. And he is given the line of least squares, the standard deviation, and the coefficient of correlation, which whatever formal truths they tell him do not give him the information he needs.

And first the secular trend. The line of least squares seems to an economist a line of least deviations weighted by their own quantity. To use as a base line from which to measure deviations a line determined for weighted deviations seems curious. Nor does the formula of the slope $\frac{\Sigma xy}{\Sigma x^2}$ bring any reassurance. That the slope should be determined by weighting the original items by the length of time from the time center is a form of weighting which has no economic meaning. Besides, it is a form of weighting which leads to absurdly unstable results. We are dealing not with hit or miss facts, but facts in sequence. And the sequence of these facts in terms of a quadrilateral field can be most generally called a straight line zigzag. Of course this is a generalization. Each series may have its own particular form. But if we combine together five unlike series—I have chosen New York Bank Clearings, Pig Iron Production, Bradstreet's Prices, Imports and Building Permits—and use the method of change from month to month relative to the corrected position in deviations of the original month, the average change from month to month where the original month was between $+.5$ and $-.5$ was only 8 per cent less than the average of months that had a greater deviation from the secular trend. Dealing with a period which includes only three or four main zigzags we are at the mercy of the positions on the zigzag which the time extremities occupy.

One test of this instability is the amount of change in the secular trend which a slight change in the group of years included produces. Starting with the slopes given in Persons' *Indices of General Business Conditions*, I found the slopes for the somewhat smaller number of included years. I used the same five unlike series. For four of them Persons' secular trend covers the years 1903–1916. For those four I found the secular trend for 1903–1913, 1904–1914, 1905–1915 and 1906–1916. For one series—Bradstreet—Persons' secular trend covered the years 1903–1914. In that case I used included nine-year periods.

The following are the results:

New York Bank Clearings—Persons' +295; My included periods +244, +82, +47, and +337.

Pig Iron Production—Persons' +95; My included periods +122, +101, +69 and +114.

Bradstreet's prices—Persons' +110; My included periods +115 +130, +123 and +92.

Imports—Persons' +70; My included periods +79, +76, +68 and +88.

Building Permits—Persons' +99; My included periods +178, +65, −11, and +25.

The secular trends of none of these series are very stable. In two of these series they are in wild conflict among themselves. Now the chief reason for this lack of stability is simply that the waves of the original series are not smoothly repetitious. But the instability is increased by this peculiar slope formula as a study of the charts on page 200 of Persons' *Indices of General Business Conditions* will make evident.

Against this conception of a line of least squares we might embody as far as possible a line of arithmetic means. The line of least squares passes through the coördinated means of the series as a whole, but, barring accidents, points on one half of the line will have an arithmetic average higher or lower than the arithmetic average of the corresponding points of the original data. And for the other half of the line, of course, the situation will be reversed. Thus this secular trend which is used as a base line for measuring the plus or minus deviations of the original data, and for indicating the least improbable future, in the arithmetic average sense slopes itself in relation to the original data. And so I would urge in its place, what has been urged before, that the secular trend be the line that passes through the coördinated means of the two time halves of the series (and thereby, of course, through the coördinated means of the time series as a whole). Points on the line of coördinated means for the two time halves have the same arithmetic

average as the corresponding points of the original data. The line can be used for predicting the future by showing the arithmetic, or if we use logarithms, geometric progress between the two time halves of the series.

Yet in using this line of coördinated means as the secular trend, certain questions arise. In the first place, I was surprised at the smallness of the difference of two secular trends calculated with such a different system of weights. Pairing the two results in the five series we have as follows: +295 and +276, +95 and +112, +110 and +120, +70 and +84, and +99 and +92. In only two of the series is the difference greater than 10 per cent. I have compared the two methods in other series, and it is rare that the difference is very great. In the second place, the secular trend of coördinated means shows almost as great instability, as tested above, as the line of least squares, although there is no progressive weighting towards the time extremities; on the other hand, the very equality of the weighting gives arbitrary results with the shifting of the time center. For a peak or depression will be swung now this side, now the other side of the time center and thus add in a fortuitous way to the average of a time half. Now such peaks or depressions near the time center would have a minimum effect on the line of least squares.

Thus the line of coördinated means solves the problem of stability little more than the line of least squares, if calculated from the original data. But if calculated from a three-year moving average, we get, of course, the stability of the mean of three secular trends of slightly shifted years. And using this moving average, don't we get a secular trend that more exactly suits our demand? We want a line that will not be disturbed too much by the quantities of the extreme years. These may represent either the accident of the particular phase of the business cycle with which the series begins and ends, or a change in the real secular trend. Both of these reasons justify a reduction of weights for the extreme years. The moving average gives them one third and two thirds the weight of interior years. Again the use of the moving average blends over both time halves the phase of the business cycle nearest the time center. Also for those who prefer the line of least squares, a moving average—preferably of five or seven years—would take a good deal of the nonsense out of its slope formula. Of course the use of the moving average qualifies one of the claims for the use of the line of coördinated means. The averages of the ordinates of its two time halves would no longer be identical with the averages of the unweighted original data. But for such difference as would exist there would seem to be sufficient reason.

Next—the unit of deviation: The unit generally in use is the standard deviation. Again I can see no justification for squaring except as a system of weighting. The question is, should deviations be weighted by their own amount in determining the unit of deviation? For that I have found no economic reason. What it amounts to graphically is that where there are a lot of monthly joggles and where the peaks and depressions are more acutely pointed standard deviation is large relative to average deviation. Statistically the question is not very serious, as the range of ratio of average to standard is probably not very great. In these five series it is 79, 86, 79, 76, and 78 per cent, respectively.

Lastly, there arises the question of the mathematical comparison of time series for similarity and lag. For this the coefficient of correlation has been largely used. It is only necessary to look at the numerator Σxy to know what kind of a likeness or unlikeness will be shown by such a formula. When peaks and troughs are in agreement, there will be a positive coefficient of correlation whether most of the rest of the two curves have random or opposing relationships. This can only be easily illustrated if we assume like quantities to all the pairs but with signs that mutually vary. In terms of average deviation assume 4 pairs of 2.5, 8 pairs of 2.0, 10 pairs of 1.5, 12 pairs of 1.0, 12 pairs of .5, and 14 pairs of .1—in short, two series which could be plotted into not unusual zigzags. If there were similar signs for the 12 pairs of 2.5 and 2.0 but a random relationship of signs for the other 48 pairs, there would be a positive coefficient of correlation of 60 per cent. Or put graphically, if peaks and troughs are in agreement though they occupy only one fifth of the time of the series, and though in the other four fifths of the time the chances are even that one series will be above the secular trend when the other is below it, nevertheless there is found a positive correlation of 60 per cent. If the 48 pairs have similar signs but the 12 pairs have a random relationship of sign, there would only be a positive correlation of 40 per cent. We might add the still more impossible case where the 12 pairs have like signs and the 48 unlike signs, one against four, but there is a positive correlation of 20 per cent. Obviously the coefficient of correlation tells us little of the likeness of the two series as a whole. It tells us only of the likeness of peaks and depressions. We guess from such likeness, the likeness of the series as a whole; but our guess is subject not merely to random error, but to the systematic error of the difference of shapes of different series.

Should we agree that the use of the coefficient of correlation is to tell us about peaks and depressions, we could be told that more clearly

by raising the power in the formula. If we use $\frac{\Sigma x^3 y^3}{\sqrt{\Sigma x^6}\sqrt{\Sigma y^6}}$ instead of $\frac{\Sigma xy}{\sqrt{\Sigma x^2}\sqrt{\Sigma y^2}}$, then carrying it through with our same illustration, like signs for the 12 larger pairs and random relationship for the signs of the 48 other pairs give a positive correlation of 91 per cent. Like signs for the 48 smaller pairs and random relationship of sign for the 12 larger pairs give a positive correlation of only 9 per cent. If the 12 pairs had like signs though the 48 pairs had unlike signs, there would still be a positive correlation of 82 per cent. There you have a formula that tells you about peaks and depressions and about nothing else. If the computation is not too difficult it should be used in place of the coefficient of correlation.

Just as important to the economist as the knowledge of the relation of peaks and troughs of one series to another is the relationship of slopes. To know the lag of the rise of prices is fully as important as to know the lag of the highest prices. To find the lag of slopes by the use of the coefficient of correlation of first differences involves the same objection which I have urged against its use with deviations from the secular trend, with this additional argument that while the correlation of peaks and troughs is an important economic relationship the correlation of rapid rise or rapid fall has no such economic import.

Instead we could use the arithmetic average of the differences between the differences of successive x and y—counting all resulting differences as unsigned. The minimum average resulting would show us the degree of lag and what relationship this average would have to random relationship could also be shown in a cumbrous way. But the question arises as to whether a difference of slope of $+.2$ to $+.6$ is as significant as the difference between $+.2$ and $-.2$. By this formula they would be given equal significance. Believing that differences of degree of rise or fall have much less significance than the difference between a clear rise and a clear fall, the natural thing to do is to group our first differences into three zones, the zones of clear rise, of little change, and of clear fall. If our original figures are average deviations carried only to the first decimal point, then the zero of first differences is a sufficiently broad zone of little change—as about 15 per cent of the cases will fall therein. Calling our three zones $+1$, 0, and -1, we can very rapidly find the lag of one series to another in respect to rise and fall by finding the minimum average of unsigned differences between first differences of paired instances classified in these three zones. Similar series would show no average difference. Reversed

series would show an average of two, if we subtract from the total number of instances the paired zeros, while series which have only a random relationship, would tend to show an average of one after carrying on the same subtraction.

Thirdly, a more general type of information, but probably of less economic significance, can be obtained by finding the unsigned average difference between paired instances in units of average deviation from the secular trend. Graphically this represents the average difference between the two sets of zigzags. Again the lag is found by finding the minimum average difference. Where two series are reversed the average difference would be two units of average deviation (not, of course, two units of standard deviation). Where two series have a random relationship, the average difference would tend to be the average deviation plus twice the difference between paired instances where the paired instances and the resulting differences have the same sign divided by the total number of pairs.

Here, also, we might raise the question of the use of a zonal method. But here I think economic reality would be against such use, as the difference between +1.5 and +.5 has as great economic significance, I think, as the difference between +.5 and −.5.

So we have three ways, each way embodying a separate idea, by which different series may be compared and the lag between them found. I was interested in comparing the results from the last two ways with that shown by the coefficient of correlation and found on page 124 of Persons' *Indices of General Business Conditions*. The two series were Pig Iron Production and Sixty to Ninety Day Paper. I have used the years 1904–1913 for the Sixty to Ninety Day Paper. Because the figures for standard deviation were easily available, I have used them. The lag is the lag in months of Sixty to Ninety Day Paper to Pig Iron. C. of C. stands for Coefficient of Correlation; Z. of S. for the zonal method of first differences; and M. D. of D. for the mean difference of deviation.

Lag.....	0	2	3	4	5	6	7	8	12
C. of C.....	.34		.67	.72	*.75*	*.75*	.73	.70	.45
Z. of S.....	.93	.68	.61	*.56*	.61	.66	.73	.76	.81
M. D. of D...	.79	.68		.58	*.55*	.57	.61	.63	.83

The three methods give three different lags. Sixty to Ninety Day Paper lags behind Pig Iron Production five to six months in its peaks

and troughs, four months in its slopes and five months in the average difference of the two zigzags. A study of the charts will confirm these results. It shows that if we are to make a numerical statement as to lag it is desirable to be specific as to the characteristic in which there is a lag. Now the two most important characteristics are the peaks and troughs and the slopes. In this case the difference of lag of these two characteristics is marked.

PRICE CHANGES IN CHINA

THE EFFECT OF FAMINES AND THE RECENT RISE IN PRICES[1]

By J. Lossing Buck, *University of Nanking*

No systematic study of prices in China covering a long period of years has been made, so far as the writer has been able to ascertain. It is a difficult thing to do because of the many different kinds of money, and because of the lack of data on prices. The best work is that of the Bureau of Markets of the Ministry of Finance, which now publishes monthly index numbers of prices of 147 commodities on the Shanghai market. This index number, with February, 1913, as the base, has been published regularly since its initiation in September, 1919. Manifestly this period is entirely too short to give us an adequate idea of the long time price movements in China. An index of 28 import and export commodities was made, for the years 1867–1922, by Dr. C. Y. Tang in his thesis presented to Cornell University. This index gives us a good picture of the past and is very suggestive, but it does not include a number of the more representative commodities in China. The prices are also port city prices which are not typical of much of China.

The writer is submitting indexes of prices of a limited number of commodities for two interior places in China. The commodities for these two inland places are among the most representative, although by no means inclusive.[2]

Attention will be first given to the index for nine of the more important commodities entering trade at the market town of Tuan Tsen Chen, in the county of Wusiang, Shansi Province, about one thousand miles inland from the port city of Shanghai and a two or three days' trip beyond the nearest railroad.

In computing these index numbers, the years 1910–1914 were taken as a base, or 100. The average price was secured for these five years and the prices for the various years were divided by the five year average price, in order to obtain index numbers for each commodity for each year. The aggregate index is obtained by adding the indices for the same year for all commodities and dividing by the number of commodities.

[1] The writer is indebted to Dr. F. A. Pearson of Cornell University for valuable suggestions in the preparation of this article.

[2] Mr. C. M. Heh and Mr. R. T. Tsui secured the original data as part of their class work at the University of Nanking, Nanking, China.

Table I gives the indexes of these nine commodities.[1] The index in 1875 was 57 per cent of 1910–1914. In 1878 it rose to 196 and fell to 180 in 1879. During these two years, one of the severest famine periods in the history of the country occurred. The index for the year 1881 was 48 and for eighteen consecutive years the index ranged from 30 to 54. In 1899 another sudden rise in price occurred, giving an index of 191. This again was due to famine and prices remained at a high level for the two succeeding years. Thereafter prices declined until the year 1910, but did not reach as low a level as prevailed previous to 1899. Beginning with 1911 there was a steady rise, until in 1923, when the index of prices was 300, with no indication of any change from this upward trend.

TABLE I

INDEX NUMBERS OF COPPER PRICES OF NINE PRODUCTS IN TUAN TSEN CHEN, WUSIANG, SHANSI PROVINCE, CHINA; 1910–14=100

Year	Index	Year	Index	Year	Index	Year	Index
1875	57	1888	49	1901	183	1914	128
1876	59	1889	43	1902	158	1915	140
1877	65	1890	43	1903	124	1916	157
1878	196	1891	42	1904	111	1917	178
1879	180	1892	48	1905	90	1918	180
1880	121	1893	39	1906	93	1919	204
1881	48	1894	37	1907	88	1920	232
1882	42	1895	48	1908	..	1921	264
1883	37	1896	46	1909	83	1922	277
1884	38	1897	54	1910	65	1923	300
1885	34	1898	50	1911	79		
1886	31	1899	191	1912	105		
1887	53	1900	183	1913	121		

The famine of 1878 and 1879 was caused by drought and hail and that of 1899, 1900 and 1901 was caused by drought. Wheat reached the peak of 343 in 1879 and 219 in 1901. Prices of other food products, millet, black soy beans, and kaoliang (a grain sorghum) reacted in the same way to the shortage of crops. Famines do not materially affect the prices of non-food products, such as white cloth, tobacco, and cotton. From 1875 to 1910 white cloth remained fairly constant. Like other products, it began the steady climb in 1911 and reached 346 in 1923. Kaoliang wine, which is manufactured from food products, increased in price during the famine years.

Table II gives the index numbers of eight commodities (in copper) at Yenshan, Chihli Province, a county seat thirty miles from the railroad and about one hundred miles south of the port city of Tientsin. The data were secured for single years by ten year periods.

[1] These are: wheat, soy beans (black), kaoliang (a grain sorghum), millet, foxtail millet, cotton, white cloth, tobacco, and wine.

TABLE II

INDEX NUMBERS OF COPPER PRICES OF EIGHT COMMODITIES AND OF THE EXCHANGE RATE BETWEEN COPPER AND SILVER AT YENSHAN, CHIHLI PROVINCE; CHINA; 1914=100

Commodity	1864	1874	1884	1894	1904	1914	1919	1920	1921	1922	1923
Farm cart	27	33	47	53	67	100	120	53	167	233	267
Plow	37	37	50	63	75	100	125	63	188	225	250
Harrow	40	40	50	60	70	100	120	60	160	200	250
Corn	25	27	33	40	73	100	140	220	180	200	207
Kaoliang	23	25	31	41	75	100	156	219	175	188	200
Wheat	25	33	35	45	85	100	165	225	160	200	255
Soybean	33	33	42	50	92	100	183	283	217	283	308
Farm wages	70	75	80	90	100	100	150	150	250	350	500
Index	35	38	46	55	80	100	145	159	187	235	280
Exchange between silver and copper	..	..	..	59	74	100	100	104	119	141	152

The prices of a food product like wheat, and a non-food product like the farm cart react differently to economic conditions in a famine stricken area. In 1920 the price of wheat rose to 225 per cent of 1914 whereas the price of a farm cart fell to but 53 per cent of the value in 1914. The index numbers indicate the marked decrease in the price of farm implements as compared with the marked increase in the price of grain during a famine year.

Prices increased during the sixty-one year period, 1864–1914, 186 per cent as compared with an increase of 180 per cent from 1914 to 1923.

The usual effects of famine on prices may be listed as follows (prices of some commodities, not included in Table I or II are needed to substantiate what is otherwise common knowledge for those acquainted with famines):

1. Food products and often fuel (especially stalks and straw) increase greatly in price.

2. Non-food products remain the same in price, or else decline.

a. Farm implements are sold to secure money with which to purchase food.

b. Lumber, and often wood for fuel, decreases in value because of the tremendous flooding of the market with supplies from the village trees, which are frequently used as a reserve for times of famine. Timber from dwellings and furniture are often placed on the market during a famine period. Clothing is also sold.

c. Labor animals are placed on the market in large numbers, first, because money is needed with which to buy food, and second, because the animals are consumers of food and fuel products.

d. Land values decrease because large numbers of farmers sell property to obtain money with which to purchase food.

e. Even human beings are sold, especially women and children, because the demand for food is so great. When one considers the sudden increase in prices of food products, about 300 per cent, during the famine 1878–79, one can more easily understand the desperate straits which make possible the breaking up of the family in this way. This 300 per cent increase, over the base period of 1910–14, does not present a true picture, because the prices were so much lower in 1875, just before the famine. Taking the year 1875 as a base, the price of wheat in 1879 increased 544 per cent.

3. The rich become richer, and the poor poorer. The rich buy the land, and, of course, reap a handsome profit when conditions return to normal. However, without the rich to buy the land and with no credit facilities, the poor farmer is fortunate if he can find any market for his land. Often a point is reached when there is no market for any non-food product.

The two striking features of the index numbers of both Tuan Tsen Chen and Yenshan are the tremendous rises in price in famine years and the very marked advances in the more recent years. The latter may be partially explained in terms of copper currency. Copper decreased in value during this period and prices of commodities given in copper coins naturally rose. Silver is the commonly accepted medium of exchange for large transactions. There is always a varying exchange rate between silver and copper coins and this rate has been increasing in terms of copper coins throughout the country.

Some of the reasons for the depreciation in copper coins and the accompanying rise in prices may be briefly surveyed as follows:

a. Fall in the silver price of copper ingots.

b. Excessive minting of copper coins.

c. Reducing the amount of copper in the coins.

In recent years the number of shops issuing paper money has also very greatly increased.

Another important factor in the rise in prices at interior points is the recent demand for grains from these producing areas. Until this demand occurred the prices were comparatively lower than at the port cities where grain entered domestic, or foreign commerce. With the development of railroads and with the increased demand for grain products, prices rose to a much higher level than occurred at the port cities.

AN EXTENSION OF THE METHOD OF MOMENTS

In an article entitled, "Growth Curves and Railway Traffic," in the December, 1924, issue of this JOURNAL, Mr. Leroy E. Peabody makes use of what he terms "the method of moments" in fitting the Gompertz and the Makeham curves which the author credits to me.[1] This reference is somewhat misleading. The method in question differs in an important feature from the method of moments as conventionally applied and was developed a year ago by Mr. Max Sasuly of the Bureau of Standards.

According to the standard method of moments a theoretical curve $y=f(x, c_1, c_2, \ldots, c_n)$ may be fitted to a given set of observations by equating the area and first $n-1$ moments of the curve to the area and first $n-1$ moments of the observations. This method thus requires as many momental equations as there are constants to be determined. One of the essentials to the application of the method of moments is that the expressions for the moments in terms of $c_1, c_2, c_3, \ldots, c_n$ must be such that we can easily solve the system of momental equations. Very often, however, we have to deal with transcendental equations which are difficult to solve. In the extension suggested by Mr. Sasuly, an extra momental equation is used to eliminate the transcendental term and to express the constants in a simple algebraic form.

The difference between the two methods may perhaps be best illustrated by reference to the fitting of the Makeham curve. This curve is selected not only because Mr. Peabody uses it in his article but because the fitting of it by the standard method of moments has already been explained by Pearson.[2]

The equation to the Makeham curve is

$$y=ks^x g^{c^x}. \tag{1}$$

Following the notation used by Pearson and taking x_0 as the midpoint of the range l of years to be included in the data and

$$s'=s^l, \quad c'=c^l, \quad g'=g^{c^{x_0}}, \quad k'=ks^{x_0},$$

we get

$$y_{x'}=k's'^{x'/l}(g')^{c^{x'/l}} \tag{2}$$

where x' is the time counted from x_0 as the origin. Hence, dropping accents, and representing logs by capital letters, we obtain

$$Y=K+Sx/l+Ge^{2\pi x/l} \tag{3}$$

where $e^{2\pi}=c'=c^l$.

As there are four constants to be determined, we must equate the area and the first three moments about the midpoint of the range l of the curve to the corresponding quantities of the observations. The four equations are:

[1] Mr. Peabody says on page 79 (footnote): "I am indebted to Mr. Schultz of the National Bureau of Economic Research for the method here given." At the time that I suggested the use of this method to the author I was on the research staff of the Institute of Economics.

[2] See *Biometrika*, Vol. I, pp. 298–303.

$$A=\int_{-l/2}^{+l/2} Y dx = l\left[K+G\,\frac{\sinh n}{n}\right] \tag{4}$$

$$A\mu_1=\int_{-l/2}^{+l/2} Yx\,dx=\frac{l^2}{12}\left[S+6G\left\{\frac{\cosh n}{n}-\frac{\sinh n}{n^2}\right\}\right] \tag{5}$$

$$A\mu_2=\int_{-l/2}^{+l/2} Yx^2dx=\frac{l^3}{12}\left[K+3G\left\{\frac{\sinh n}{n}-\frac{2\cosh n}{n^2}+\frac{2\sinh n}{n^3}\right\}\right] \tag{6}$$

$$A\mu_3=\int_{-l/2}^{+l/2} Yx^3dx=\frac{l^4}{80}\left[S+10G\left\{\frac{\cosh n}{n}-\frac{3\sinh n}{n^2}+\frac{6\cosh n}{n^3}-\frac{6\sinh n}{n^4}\right\}\right] \tag{7}$$

From these Pearson obtains, after some reductions, the following transcendental equation from which to determine n.

$$e^{2n}=\frac{(\beta+2)n^3+3(\beta+4)n^2+3(\beta+10)n+30}{(\beta-2)n^3-3(\beta-4)n^2+3(\beta-10)n+30} \tag{8}$$

where $$\beta=\frac{4(20\mu_3/l^3-3\mu_1/l)}{(12\mu_2/l^2)-1}.$$

As the solution of this equation to the required degree of approximation is not an easy task, Mr. Sasuly would eliminate the transcendental term entirely through the use of the fourth moment:

$$A\mu_4=\int_{-l/2}^{+l/2} Yx^4dx=\frac{l^5}{80}\left[K+5G\left\{\frac{\sinh n}{n}-\frac{4\cosh n}{n^2}+\frac{12\sinh n}{n^3}-\frac{24\cosh n}{n^4}+\frac{24\sinh n}{n^5}\right\}\right] \tag{9}$$

in connection with equations (4), (5), (6), and (7). If this extra moment be introduced we obtain the following simple algebraic expression for n:

$$n=\frac{2\beta_1}{1-\beta_2} \tag{10}$$

$$\beta_1=\frac{a_3-a_1}{a_2-a_0},\quad \beta_2=\frac{a_4-a_2}{a_2-a_0},$$

$$a_0=A/l,\quad a_1=A\mu_1\frac{12}{l^2},\quad a_2=A\mu_2\frac{12}{l^3},$$

$$a_3=A\mu_3\frac{80}{l^4},\ a_4=A\mu_4\frac{80}{l^5}.$$

It is this extension or modification of the Pearsonian method which Mr. Peabody used in fitting both the Gompertz and the Makeham curves.

The discovery that the use of an extra moment will eliminate the transcendental term is the outcome of a search on the part of Mr. Sasuly for a method of fitting the exponential $y = be^{-mx}$ without having to reduce it to its logarithmic form and without having to solve a transcendental equation in m. When the present writer called his attention to the fact that the negative exponential is one of the "uncommon types" of Pearson's system of frequency curves and that in fitting this curve by the method of moments Pearson has recourse neither to logarithmic transformation nor to the solution of a transcendental equation,[1] Mr. Sasuly, after an examination of Pearson's method, came to the conclusion that *any* transcendental term may be eliminated through the use of an extra moment. At the time that Mr. Peabody's article was being prepared, the scope and validity of this extension of the method of moments were only partially investigated which is one of the reasons why the publication of this method was deferred.

Mr. Peabody finds that the less involved summation method gives more satisfactory results than this (modified) method of moments. This is due either to the use of raw moments or to the comparatively large probable error of the (extra) fourth moment. Should this finding be confirmed by a wider experience with the two methods, we will, of course, have to conclude that the advantage of using an extra moment is of more theoretical than practical importance. For the time being it is best to suspend judgment.

HENRY SCHULTZ

Washington, D. C.

FORECASTING SECURITY PRICES

On April 17, 1925, a dinner meeting of the American Statistical Association was held at the Aldine Club in New York City. About 420 people were in attendance. The topic under discussion was Forecasting Security Prices.

The first four speakers represented commercial agencies active in the forecasting field. Mr. Paul Clay of Moody's Investors' Service ascribed the success of that institution to its creed. He affirmed a belief in economists and statisticians because "they are far ahead of the average business man in realizing the practical value of economic study, and they have done wonders in learning to understand the ways of trade and in reducing its hazards." He continued:

> The free play of economic forces was never better safeguarded. This country was never more free from actual monopolies. The stock market was never more unspotted by manipulation, and bulls and bears never before had so little to do with your own investment success as they have now.
>
> We have never seen the infallibility of judgment which is supposed to belong to the mythical insiders. To our mind, the belief in inside interests is as fabulous and antiquated as that in the oracle of Delphi.

[1] The exponential may be written $y = \frac{N}{\sigma} e^{-x/\sigma}$ where N is the total frequency and σ is the standard deviation. See Elderton, W. P.: *Frequency Curves and Correlation*," Addendum, 1917, p. 8, and *Phil. Trans., A*, vol. ccxvi, pp. 429–457.

We believe in the rule of economic forces, and this is why we reject the Delphic oracles and inside interests. There is nothing mysterious about the stock market except those phases of it which research and data have not yet covered. It is merely the great American capital market, into which flows the surplus capital of every industry, and out of which flow streams of capital to supply the expansion needs of every industry. Typical men, whatever their business and wherever they live, buy stocks and bonds when they have surplus funds, and not at other times. They dump securities on the markets when they are in financial distress, and they never do so when they are not in distress. Therefore, the financial situation of the typical business man dominates the course of the market.

So it is that the stock market is a creature of every day economic forces. Complete data would probably show that during bull movements the rise is roughly proportionate to the inflow of capital saved out of the earnings of industries. It would probably show that during bear movements the decline is proportionate to the urgent liquidation on the part of business men who are short of necessary capital. Presumably, if we had an accurate measure of the surplus earnings of industries in prosperous times and of the capital shortages of industries in depression times, we should know exactly what the stock market is going to do.

Our researches have disclosed many evidences of the rule of economic forces. Perhaps the most striking is the parallelism of our trade barometer with the main swings of the stock market. This barometer is a monthly weighted average of selected barometrical trade returns; and we have compiled it in either finished or in crude form back to 1863. Throughout the entire course it runs generally parallel to the main swings of the stock market.

Close study reveals the fact that the stock market is a creature of trade, and a trailer after it, and not a leader or a maker of trade conditions. The popular belief that the stock market discounts or anticipates has no basis in fact. The belief itself doubtless arises from the fact that trade conditions are so complex that very few people realize changes therein until months after these changes have occurred.

The high peaks of the stock market are the result of an unbroken chain of economic forces. First, the money value of our commercial turnover dominates the trend of interest rates; second, commercial paper discounts move with interest rates, and bond yields follow commercial paper. The high peak in the stock market is that point at which prices have risen so far that stock yields are slightly below bond yields. This has been consistently true of the major peaks in the stock market throughout the history of the New York Stock Exchange.

Seasonal variations of trade and stock prices are practically alike except in that portion of the year during which the money market exercises more influence upon securities than the trade situation does. From the middle of September to the middle of January, the stock market responds to the money market curve, whereas during the entire balance of the year it responds to the growth of trade conditions. Furthermore, the money market curve itself is the creature of the trade curve.

Now, if the stock market is a creature of economic forces, it follows that the right method of forecasting is to study these forces. By way of defining the right method, let me first speak of some of the methods which we have rejected. First, there is the single index method. Many years ago it was believed that pig iron was a good forecaster for the stock market, and now many persons think that one can forecast successfully just through observation of blast furnace capacity, and nothing else. There is no question that these indices are worth studying, but we do not believe in any single index, for the course of the stock and bond markets is a reflection of the change in the financial condition of the entire American people. If the resultant of forces can be calculated from a single factor in finance, why not in physics, as well?

Another forecasting instrument which we have rejected is the automatic barometer. There are those that, by combining certain barometrical returns, obtain a barometer or index number which is supposed to move ahead of the stock market and indicate its course. This surely is better than the single index, but here again there is the serious objection that the forecaster is compelled to reject all economic forces not contained in that barometer.

Doubtless, however, the most popular method of forecasting is chart reading, and this, too, we have rejected. The chart is a post-mortem and not a diagnosis, it is a result, rather than a cause, and its occurrence is afterward rather than beforehand. Prices, according to John Stuart Mill, are founded upon supply and demand, and not upon double tops and double bottoms. Adam Smith believed that the causes of value

were utility and labor, and not bull and bear chart curves. We have chosen to cast our lot and our fortunes with these sober students of economic forces.

Now, since the course of the stock market is the resultant of economic forces, let me tell you what some of these measures are and how we use them. First, there is our trade barometer, which is a weighted average of barometrical trade returns. Trade conditions are bullish or bearish in accordance with the movement of this trade barometer, except that an excessive rise far above normal is bearish, while an excessive decline far below normal is bullish.

Another measure of economic forces which we highly value is our employment index. Its value lies in the fact that it is a crude measure of the purchasing power of the American people. When labor is well employed, plant capacity and capital is also well employed, and employers as well as their men are making money.

A third important measure of economic forces employed by us is the ratio of bank loans to deposits. A sharp rise in these ratios invariably means that capital supplies are running short, and vice versa. Any doubt as to interpretation can readily be eliminated by use of our monthly averages of interest rates.

Another of our measures of economic forces is our inflation index, which is an abstract index number so constructed that it will rise sharply whenever credit, inventories or operating costs are inflated. By inflation we mean not mere expansion, however great, but any condition of unsoundness or overexpansion which is bound to result in forced liquidation on the part of business men.

Of our other measures of economic forces, I will mention only one. This is an index number of the latent earning power of industries. By latent earning power, I mean the earnings which might theoretically be made if materials, labor, capital, etc., could be bought at current prices, instantly converted into finished goods, and the goods instantly sold. In our woolen index, for example, we figure just as a woolen mill manager might. We compute the cost of production, and compare with the market value of the finished goods.

A few applications of these measures of economic forces will serve to illustrate our methods of forecasting. At the present time, for example, we are not taking a bearish position. On the 5th and 12th of March we advised profit taking, merely because stocks had risen too fast as compared with trade improvement, and the market was in an overbought condition. However, these measures of forces have not turned bearish, and we have regarded the decline which occurred since the middle of March as nothing worse than a fair opportunity to repurchase stocks at substantial concessions.

Mr. Clay further pointed out that in 1920 forecasts of his service were based primarily on the overexpansion of credit. In the summer of 1921, they were dominated by the latent earning power of industries. In October, 1922, the employment index was the governing factor, while in December, 1924, reliance was placed mainly upon the employment index supplemented by the ratio of loans to deposits.

Mr. Clay concluded his remarks with a plea for more extensive research in this field.

The next speaker of the evening was Mr. William Peter Hamilton, editor of the *Wall Street Journal*. He set forth the theory of the stock market, original with the late Charles H. Dow, former editor of the *Wall Street Journal*. He said:

The major swing of the stock market is the most important of the three tendencies into which Dow's theory divides the price movement. Over a period of twenty-one years, starting from June, 1900, and ending with June, 1921, there were twelve major movements of the stock market—six up and six down. What is interesting to note is that the upward movement, or bull swing, averaged twenty-five months in length, while the downward, or bear market, averaged seventeen months.

It is to be noted that these bull and bear markets are never uninterrupted. Here comes in what may be called the secondary movement of the Dow theory. A bull market is interrupted by sharp reactions, as, for instance, that which set in on the 7th of last March, reaching its low point on March 30, while a bear market is checked in its progress by sharp recoveries, if only for the technical reason that such a market is apt to become oversold.

That is the Dow theory of the price movement. It does not claim to be anything more than an hypothesis, and practically every scientific discovery the world ever saw developed from a workable hypothesis.

The speaker expressed the view that the present reaction of the stock market was merely a minor phase of a general upward movement which was not yet over. Mr. Hamilton pointed out that he had published a book in 1922, called *The Stock Market Barometer*, showing the application of the principles of the Dow theory. He stated that if the Harvard University Committee on Economic Research were to omit bank debits from its line of speculation, that this line would correspond to the Dow-Jones averages used by the *Wall Street Journal*. He stated that the line of speculation always precedes by some months a corresponding fluctuation in the line of business. Mr. Hamilton emphasized the fact that manipulation of the market had little significance. He stated, "It is not difficult to see how, in a narrow market with a small volume of trading, the daily fluctuation could be manipulated. It would be much more difficult to manipulate the secondary reaction or rally, because it would be necessary to take in hand at least a majority of forty active stocks. You may take it from me that there is no financial interest, or combination of financial interests, in Wall Street or in the whole country, which is big enough to do anything of the kind or foolish enough to try."

Mr. Hamilton cited as an example of the surprising way in which the stock market forecasted business that in December, 1917, eleven months before the armistice, the market predicted the final and complete defeat of Germany. Mr. Hamilton vigorously condemned "stock tipping agencies."

His remarks were followed by those of Roger W. Babson, of Babson's Statistical Organization, who illustrated his remarks by the aid of a chart, showing the equality of action and reaction in business for a long period of years. He held that while it was folly to attempt to forecast the short swings in the stock market, practically all the economic services have a clean record on the long-swing movements. He said that we know what period we are in and what the next period will be; the only uncertain factor being the duration of the respective periods. As the Department of Commerce and other organizations are continually collecting and compiling more and more statistics, all the services are constantly improving their ability to determine the duration of the periods. He pointed out that the science of forecasting was rapidly developing into a very sound and useful industry, and that every additional person who becomes interested in this study makes the next boom less reckless and the next panic less severe. Mr. Babson predicted that the forecasting service of years hence would combine the good features of the leading agencies that now exist. He also expressed the view that we are still in the upward movement of the stock market.

The fourth speaker was Mr. Ray Vance, President of the Brookmire Economic Service. He began by defining the term, "forecasting" as being "such a measure of business happenings and such a survey of the results which have followed these happenings in the past, as will permit the anticipation of security price movements with a worth-while approximation to accuracy." He emphasized the fact that by security price changes he did not mean the changes which occurred from hour

to hour, or even from month to month, but those major movements which ordinarily occur not oftener than once a year. He also stressed the point that the Brookmire method cannot be regarded as in its final form. Changes of major importance may be made at any time. He stated that the factors used and the reasons for using them are:

1. The height of stock prices themselves and the activity on the exchange. The higher stocks are, the more likely (other things being equal) they are to decline, and incidentally, the greater the activity, the more likely is a decline.
2. The greater the volume of basic raw material production going on within this country, the greater is the chance that production is running ahead of consumption. Therefore, high physical volume of production is used as a bearish factor, and low physical volume as a bullish one.
3. The importation of goods is, as an economic fact, an equivalent to an increase in production and vice versa. Therefore, the ratio between imports and exports is used as a third factor. When this ratio is high, it indicates trouble, and when it is low, it shows a fundamentally strong position.
4. The turnover of bank deposits is measured by the ratio of bank clearings to bank deposits. A high rate of turnover is unfavorable and a low rate favorable.
5. The supply of purchasing power in the form of bank deposits is a flexible thing until the banking strength of the country is exhausted, or until a governing body, such as our Federal Reserve Board, artificially tightens credit. As an index of whether this point has been reached or not, we use the commercial paper rate in the United States.
6. Because the supply of loanable bank funds is really an international affair, we used as a sixth factor the open market money rate in London.

Mr. Vance pointed out that while this method has fallen considerably short of 100 per cent accuracy, that it has been very successful during the past two years. Recent changes in the method are expected to make it even better than it has been. He submitted a chart on which were marked the points at which clients of the service had been advised to buy and sell stocks, and he emphasized the fact that all predictions made by the Brookmire Service were specific. He stated that in February advice was given to sell stocks, and that there was no reason to believe that this advice was not sound. He concluded his remarks by saying, "We consider the service, in spite of its lack of present perfection, has accomplished far in excess of what is accomplished by the average buyer of securities, and hence its use is justified."

The program was closed by Mr. Frederick R. Macaulay of the National Bureau of Economic Research, who gave a critical review of the remarks of the preceding speakers. He began by pointing out that the Dow theory of forecasting endorsed by Mr. Hamilton stood in contrast to the other three theories in that it was based upon the movements of the stock market itself. He observed that there was a striking similarity between the fluctuations of the stock market and those of a chance curve which may be obtained by throwing dice. Everyone will admit that the course of such a purely chance curve cannot be predicted. If the stock market can be forecast from a graph of its movements, it must be because of its difference from the chance curve. There undoubtedly are such differences, said Mr. Macaulay. Because of the earning power of the companies themselves, for example, it is certain that the curve will never reach the zero line and will never rise indefinitely. There is no limit to the distance which a chance curve may rise or fall. Mr. Macaulay expressed the view, however, that Mr. Hamilton's method of prediction was much less dependable than those used by the

other three speakers, inasmuch as the essence of the Dow method was to buy as long as the market continued to rise and sell as long as it continued to fall, which was just the reverse of taking advantage of the fact that the market has a definite bottom and that there is a limit to the distance to which it can rise. Mr. Macaulay also suggested that it might be unwise, in attempting to forecast from a record of the market itself, to exclude the volume of trading from consideration. He called attention to the fact that "double tops" and "double bottoms" seem to be just as characteristic of a chance curve as of the stock market record, and hence that it is doubtful whether they have any particular signification as predicting factors.

He next turned his attention to the methods used by the other three agencies; namely, that of utilizing recorded movements of economic forces outside the markets to show what is going to happen to stock prices. He raised the question, in regard to Mr. Babson's chart, as to whether it was not just as difficult to predict the areas above and below the trend, as to forecast the course of the stock market directly. Mr. Macaulay showed that the chief way in which the method used by Mr. Clay differed from that used by Mr. Vance was that Mr. Vance attempts to construct a mathematical formula for weights, while Mr. Clay uses a mental rather than any mathematical weighting. Mr. Clay, moreover, takes into consideration any event or force which he deems important, while Mr. Vance tends to restrict himself to the six factors in his barometer. The apparent advantage of utilizing all types of forces is perhaps more than offset by the danger that the forecaster will be unduly influenced by events of the moment, and may therefore use weights that are not logical.

The meeting was ably presided over by Mr. Walter S. Case, President of Case, Pomeroy & Co., who stated that the experience of his firm had favored the use of outside economic factors in foretelling the general course of the market. As to the present outlook for the prices of railway stocks, he was inclined to agree with the first three speakers, who believed that the rising market would continue, even though he had great respect for the method used by Mr. Vance which gave contrary indications.

THE COST OF LIVING INDICES OF THE N. I. C. B. AND OF THE B. OF L. S.

In her extended article in the December number of the JOURNAL, Miss Elma Carr of the Bureau of Labor Statistics begins her valuable description of the differences between the cost of living indices of the Bureau and the Board with a reference to their extensive use in wage adjustments and ends with the conclusion that the methods of the Bureau are "far superior" and that its index should therefore be considered "much more reliable." If the JOURNAL'S space permitted, and if the subject were important enough not to have been forgotten since Miss Carr's article appeared, a great deal could be said about it. It is possible, however, to venture only a few general comments.

It should be obvious by this time that the methods and results of cost of living studies will inevitably differ; that in themselves these differences prove little about the "reliability" or absolute value of any of them; and that their value at best is altogether relative to the scope and intention of the study in question. It should be equally clear, after the experience of the past few years, that cost of living figures have no necessary pertinency to the adjustment of wages, and that they are at best no more than a general guide to the course of a certain arbitrary group of retail prices. The Conference Board, at least, does not feel that either the uses made of its cost of living index by others or the abstract validity of cost of living figures in themselves warrant magnifying the differences between the two indices in question or attaching any such significance to these differences as Miss Carr has done. Until a firmer basis is developed for the measurement of consumption, living standards and prices, and likewise for the adjustment of wages, any such invidious and unreflective comparisons as Miss Carr has made can be considered only as a kind of stone-throwing spree in a village of statistical glass houses.

The differences between the Conference Board's and the Bureau's results are small, and they merely indicate the latitude that exists for the exercise of different judgment, preference and facilities in handling the material in this field. It is far more remarkable that the Bureau, the Board, and the Massachusetts Commission on the Necessaries of Life, working independently in so vague and vast a field, and with so many differences in the detail of their method, should present so similar pictures of the course of retail prices than that flaws may be found in the procedure of any of these organizations. The Conference Board believes that, assuming accuracy and good faith in compilation, each index, though in the nature of the case not perfect, fairly reflects what it attempts to, and that, within the limits of validity which all such statistics have, each has its value and its place. That is all that "reliability" can mean in such matters.

As cost of living indices are so widely quoted and so freely, if not wildly used, a clear understanding of their nature and basis is important, if for no other reason than that such comparisons as Miss Carr's may not be taken too seriously. Accordingly, several months ago the Conference Board began to prepare a careful analysis of the indices of the Bureau, the Massachusetts Commission and the Conference Board, setting forth in detail the basis and method of compilation of each. This volume is now in press, and the Board, by way of passing the salt, gladly refers to it everyone who wishes to find out what the cost of current living indices mean and exactly how they are made in order to soothe or exacerbate any anxieties about their "reliability" which Miss Carr may have inflicted upon them. This volume affords a detailed and corrective explanation of all the points Miss Carr raises in her article. Pending its appearance it may not be amiss to indicate a few considerations which may help to drown out the faint undertone of axe-grinding which echoes through most discussions of this matter.

In the first place, it is unfortunate that while Miss Carr's description of the basis and method of the Conference Board's index is substantially accurate so far as it goes, it so seriously sins by omission and innuendo. Certain defects of the

Board's index are stressed while equally real weaknesses in the Bureau's are slurred over. There is the unfortunate implication in reiterated passages in the article that certain features of the Board's methods are mysterious trade secrets or sleight of hand, while everything about the Bureau's index is known to the public and is based on the tables of scientific law handed down from some statistical Sinai. There is, of course, nothing about the Board's method which it is not perfectly willing to tell, but it has no more found it necessary to publish a great many relatively unimportant technical details than the Bureau has. Furthermore, both the Bureau and the Board, as well as anybody who attempts to make a cost of living index, must at certain points rely upon guesses, or in the language of statistical diplomacy, estimates. Both the Bureau and the Board have done a considerable amount of estimating in various particulars and will probably have to continue to do so until the statistical millenium arrives. The only test of the validity of the estimates is in the consistency of the general results and in the amount of experience and good judgment that lie behind them. In all these respects, as the volume referred to above will show, it is hard to make a case for the "superiority" of either index.

Secondly, since it is not feasible for any agency, even the government, to price at frequent intervals a complete family budget but only samples selected as representative of each large item in the budget, it is inevitable that some assumptions or arbitrary judgments be made in such selections and that the assumptions and judgments of the different investigators will differ. Accordingly there are inevitable differences in the representative goods and services priced by the Bureau and by the Board, and these differences play their part in the end results. These are fully discussed in the forthcoming volume referred to above. If they indicate anything more than the dominant influence of the judgment and experience of the investigator in cost of living studies, anyone is welcome to the scientific residue. It has always been hazardous and useless to split hairs over "reliability" in matters of this kind.

Aside from these considerations, there are two more general distinctions between the Board's and the Bureau's indices which are perhaps susceptible of more scientific evaluation. These are the question of the basic budget used and of the coverage and method of collection of data. The expenditures which form the basis of sampling and weighting in the Bureau's index refer to some part of the year 1918 or 1919 and to conditions in shipbuilding and other large industrial centers which were acutely affected by war price conditions and where wage increases allowed an unusual surplus for goods not ordinarily purchased. The Conference Board budget on the other hand was devised eclectically out of prewar budgets which were considered representative of general and normal conditions in July, 1914. Much can be said for and against both, but probably neither is now valid for measuring present conditions; 1919 is as legendary as 1914, and the relative blackness of pot and kettle is now an idle question.

Finally, and most important, it should be remembered that the Board's and the Bureau's indices have quite different intentions and serve quite different purposes and that these differences are reflected in the coverage and method of collection of data. The Board is interested only in securing a general measure of changes in

the cost of living for the country as a whole which shall reflect conditions in all kinds of communities in all sections of the country, while the Bureau's interest is primarily to make up indices for a number of important industrial cities, and its so-called index for the country as a whole is merely a by-product of these local indices. The Bureau's index is based on quotations secured in thirty-two large industrial cities, the total population of which was 21,072,089 in 1920, except for the food index, which covers fifty-one cities with 24,466,796 population. The Conference Board's number is based on a varying number of cities for the different major items. The average for shelter since 1920 has been 167, for clothing 76, and 59 for coal. The aggregate population of these, in 1920, was 33,204,354 for rents, carefare and gas; 26,849,540 for clothing; 25,890,910 for fuel. Other sundry items, as magazines, cost of insurance, candy, tobacco, while based on a few reports, apply to the country as a whole. It seems obvious to the Board that geographical location and size of the cities affect the cost of coal, to some extent the cost of food and that local conditions are almost exclusively responsible for rent changes. Clothing price changes, on the other hand, are more nearly a matter of buying and selling policies than of geographical location. The cities included by the Bureau are large industrial centers most of which were built up greatly during the war and in which war price inflation has carried over to a considerable extent into peace times. Since the Bureau's index number up to December, 1917, was based on eighteen cities where for the most part the increases in the cost of living was greater than for the country as a whole, and since the method of obtaining the number involves a chain system whereby to increases already ascertained the next succeeding increase or decrease is added, it is evident that the effect of these early tremendous increases may be carried through the series unless the downward movement in these places was more significant and marked than in the country as a whole.

It seems to the Board, furthermore, that an investigation designed to reflect general changes in the cost of living in the country as a whole requires and is well adapted to the questionnaire method. As to whether the questionnaire or the agent method of investigation is in general the better is again a matter of preference and facilities. In view of the close similarity of the results it seems hardly justifiable to ascribe any tremendous influence to the method of collecting the data. The Conference Board has found, however, in its experience with a number of local investigations of the cost of living made by field agents, that securing information in this way has its potential pitfalls. It does not feel that the agent method insures the maintenance of a comparable standard of merchandise or that it leaves nothing to the discretion of the agent. In the case of the Bureau the same agent does not visit the same place in succession, and there is therefore considerable opportunity for variation in standards. Furthermore, the Board's experience has convinced it that for certain purposes, including cost of living investigations, the questionnaire method elicits much better coöperation and more carefully considered material than do agents, who are not always diplomatic, who have to get answers, sometimes to difficult questions, when they call, and who frequently irritate and stimulate the hostility of the victim.

Collecting data by mail is, of course, far from infallible, but it has the advan-

tage of permitting the merchant to fill in the required information in peace, at his leisure and with the guidance of his previous quotation for each item, and puts upon him who knows best the responsibility for seeing that standards of comparability are maintained. Its greatest advantage is that it permits the collection of data at more frequent intervals over a far wider area than is the case with agents. This is what the Conference Board is primarily interested in doing. A monthly index covering a wide territory could hardly be attempted except through questionnaires. The Bureau, which, by the way, collects about two-fifths of its information by questionnaire, can cover the data for only 32 cities only once every three months.

When all of these things are considered, the Conference Board feels that both series of index numbers have their value and that for what each purports to represent each is substantially correct as far as existing methods of measuring the cost of living go. The Board exercises every possible care in preserving the comparability of its data and the accuracy of its computation, and, on the basis of its experience in local cost of living surveys as well as of all other pertinent material, is zealous in revising and improving its index in order to make it better represent what it is intended to. If the Bureau does the same, as it doubtless does, those interested in this sort of thing are fortunate in having two series of index numbers of living costs, different in scope and character but similar enough in their general results to serve as a valuable check on each other and to justify confidence in both. Any other conclusion about them may be suspect of cavil, which is perhaps a favorite indoor sport of the statistical fraternity, but an abomination in practical affairs.

VIRGIL JORDAN

National Industrial Conference Board

ON THE STANDARD ERROR OF ESTIMATE

In his article on "The Measurement of Correlation and the Problem of Estimation," [1] Dr. F. C. Mills distinguishes three types of equations which may be used in estimating the values of one variable from the observed values of a correlated variable. These type equations are: $Y=f(X)$; $\log Y=f(X)$; $\frac{1}{Y}=f(X)$, where $f(X)$ denotes a parabolic function of X. In testing on concrete examples the estimates based on these equations were found to differ not so much in the actual values as in the standard errors which measure the reliability of the estimates. The following is intended as an analysis of the properties of the standard errors corresponding to the three type equations.

If the estimate based on the equation $\log Y=f(X)$ be denoted by $K^{\log Y}$ and the standard error of this estimate by $S_{\log Y}$, the upper limit of the first positive zone of estimate is, then, $K^{\log Y+S_{\log Y}}$, while the lower limit of the first negative zone of estimate is $K^{\log Y-S_{\log Y}}$. The magnitude of the positive standard error is

$$K^{\log Y+S_{\log Y}}-K^{\log Y}=K^{\log Y}(K^{S_{\log Y}}-1). \qquad (1)$$

[1] This JOURNAL, September, 1924.

The absolute magnitude of the negative standard error as derived by a similar operation is

$$K^{\log Y - S_{\log Y}}(K^{S_{\log Y}} - 1.) \tag{2}$$

From expressions (1) and (2) it is clear that (a) both the positive and the negative zones of estimate become wider with an increase in $K^{\log Y}$, the estimated value; (b) the absolute magnitude of the positive standard error is always greater than that of the negative standard error, since

$$K^{\log Y} > K^{\log Y - S_{\log Y}}$$

($S_{\log Y}$ is, of course, always positive).

By presenting the standard error as a coefficient of $K^{\log Y}$, it can be shown that the positive standard error is a larger fraction of the estimated value than the negative error. Thus, the positive standard error may be written as $(K^{S_{\log Y}} - 1) \times K^{\log Y}$, and the negative standard error as $\frac{K^{S_{\log Y}} - 1}{K^{S_{\log Y}}} \times K^{\log Y}$. It is evident that $K^{S_{\log Y}} - 1$ is greater than $\frac{K^{S_{\log Y}} - 1}{K^{S_{\log Y}}}$, because $K^{S_{\log Y}}$ is larger than unity (remembering again that $S_{\log Y}$ is positive). Since $S_{\log Y}$ is constant, the expressions above show the standard error to be a constant coefficient of the estimated value.

If the equation $\frac{1}{Y} = f(X)$ is used, the upper limit of the first positive zone of estimate is $\frac{1}{1/Y - S_{1/Y}}$, and the lower limit of the first negative zone of estimate is $\frac{1}{1/Y + S_{1/Y}}$. The magnitude of the positive standard error is, then,

$$\frac{1}{1/Y - S_{1/Y}} - Y = \frac{YS_{1/Y}}{1/Y - S_{1/Y}} = \frac{Y^2 S_{1/Y}}{1 - YS_{1/Y}}. \tag{3}$$

The absolute magnitude of the negative standard error as similarly derived is

$$\frac{Y^2 S_{1/Y}}{1 + YS_{1/Y}}. \tag{4}$$

From expressions (3) and (4) the following conclusions may be drawn:

(a) The absolute magnitudes of the standard errors of this type are functions of Y, the estimated value. Moreover, they are increasing with an increase in Y; this is inferred from the fact that the first derivatives of expressions (3) and (4) with regard to Y are positive.[1] Again, it is clear that the positive error grows

[1] The derivative of the positive error is $\frac{YS_{1/Y}(2 - YS_{1/Y})}{(1 - YS_{1/Y})^2}$, and the derivative of the negative error is $\frac{YS_{1/Y}(2 + YS_{1/Y})}{(1 + YS_{1/Y})^2}$. Both of these are positive because they are products of positive factors. Thus, $YS_{1/Y}$ is positive when Y is positive, which it is for all cases where it stands for real quantities such as prices, stocks, output, number of persons, etc., or for ratios. Again, $(2 - YS_{1/Y})$ is positive since it is larger than $(1 - YS_{1/Y})$, which we know to be positive by reason of its appearing in the denominator of (3), the expression for the magnitude of the positive standard error.

more rapidly with an increase in Y than the negative error. Thus, the ratio of the magnitude of the positive standard error to that of the negative is $\frac{1+YS_{1/Y}}{1-YS_{1/Y}}$; since $YS_{1/Y}$ is positive for many practical cases, every increase of Y makes the numerator larger, the denominator smaller and the value of the whole ratio larger.

(b) The absolute magnitude of the positive standard error is greater than that of the negative, because, while the numerators of expressions (3) and (4) are the same, the denominator of (3) is less than that of (4). As shown above, the larger Y, the estimated value, the wider is the positive zone of estimate as compared with the negative zone.

The magnitude of the standard error may also be presented as a coefficient of Y, the estimated value. The positive error appears then as $\frac{S_{1/Y}}{1/Y-S_{1/Y}} \times Y$ and the negative error as $\frac{S_{1/Y}}{1/Y+S_{1/Y}} \times Y$.

These expressions indicate (a) that the standard error represents an increasingly larger fraction of the estimated value as we pass from small to large values of Y. (For as Y increases the coefficients become larger, because their numerators do not change while their denominators shrink.) (b) That the positive error represents a larger fraction of the estimated value than the negative error.

The following is a summary of the properties of the different types of standard error of estimate:

Type of estimating equation	Magnitude of Standard error		Ratio of the magnitude of positive standard error to that of the negative error	
	As absolute quantity	As fraction of the estimated value	Value of ratio	Change in ratio with an increase in Y
$Y=f(X)$	Constant	Decreasing with an increase in Y	1	No change
$\log Y=f(X)$	Increasing with an increase in Y	Constant	Greater than unity	No change
$\frac{1}{Y}=f(X)$	Increasing with an increase in Y	Increasing with an increase in Y	Greater than unity	Increasing

Solomon Kuznets

Columbia University

PROFESSOR YOUNG'S REVIEW OF "GERMANY'S CAPACITY TO PAY"

When an economist ventures to write upon economic issues of a highly contentious nature, he naturally expects no small measure of disagreement. In the reviews of lay writers he may indeed even count with some confidence upon finding his arguments misstated and perverted and his conclusions distorted. But in scientific journals one expects to find at least a fair statement of the author's case and of the points at issue. It is obvious that unless a review does present the author's argument or thesis correctly it does not contribute to the advancement of knowledge and should find no place in the literature on the subject. In these days of rapid reviewing minor misstatements and omissions should no doubt be taken as a matter of course; but when an author's position on fundamental issues is wholly misrepresented, a decent respect for his subject leaves him no alternative to making corrections,—particularly when that subject is one of such continuing importance as the reparation problem.

In reviewing *Germany's Capacity to Pay*,[1] Professor Allyn A. Young, while endorsing the volume in the large, devotes the bulk of his analysis to expressing his disagreement on two major points of theoretical import. The first point is that the authors concern themselves only with *economic* payment and give no consideration to *legal* payment. The second point is that the authors attach insufficient importance to the leading rôle played in the progress of German inflation by the unremitting pressure for foreign payments. Now the failure of the authors to give attention to these points would constitute an important indictment of the book, if true. If, on the other hand, these points have been given specific and thorough consideration, it is the review which is indicted. We shall have to let the facts speak for themselves.

With reference to the first point, Mr. Young levels his objection against Chapter I. At the time we were writing the general doctrine was rife, particularly in business and political circles, that Germany could pay by delivering securities and that the export surplus doctrine was a complete fallacy. The purpose of Chapter I was to prove that *in the long run* the actual payments—economic payments if you please—would have to come out of an export surplus (favorable balance of payments). On page 11 we state that "year in and year out, over a long period of years, Germany cannot possibly pay a sum greater than that by which total exports exceed total imports." On page 16, line 5, we say that the export surplus is the measure of what a nation can "year after year actually pay abroad"; and again, line 20, we conclude that "over a period of years a nation cannot pay more than the amount of the export surplus." Observe the reiteration that we were concerned with stating the general principle that *in the long run* the total of real payments would be governed by the amount of the export surplus.

Now, if this had been all that we had said in the book on this general subject it is conceivable that a reviewer might have failed to note that the principle we had stated applied to the long run only. This was, in fact, only the beginning of

[1] This JOURNAL, June, 1924.

our discussion. In the opening paragraph of Chapter III, we deliberately challenged our own previous statement about the export surplus with a view to making certain that no reader would miss our point. In that paragraph we called attention to the fact that in the first chapter we had insisted that there must be an export surplus, while in the second chapter we had shown that without an export surplus, Germany had paid very considerable sums in reparation. At the end of the paragraph we asked: "Is there not something wrong, therefore, with the principle enunciated in the first chapter?"

The bulk of Chapter III is then devoted to a discussion of the means by which a nation may *temporarily* make payments abroad without a favorable balance of accounts. We have a detailed discussion not only of the transfer of foreign investments and movable property, but also of the sale of paper marks and of domestic bonds and securities. Our conclusion was that all of these *temporary* devices have been practically exhausted and that henceforth practically all German payments would be contingent upon the existence of an export surplus. We again returned to the point at issue in our chapter on how France met the indemnity of 1871. We there showed that France had made acceptable (legal) payments by the transfer of foreign bonds and other capital assets; and we again referred to the German problem (page 233), using italics so that our point would not be overlooked, as follows: "All of these means of payment (*i. e.*, by transferring capital assets) have, however, now been practically exhausted."

Rather than ignoring the possibility of making legal payment by the transfer of securities, we had gone thoroughly into the discussion of the whole problem and revealed the limitations of the method. At the time we were writing, as we point out on pages 18 and 19 of Chapter I, Germany had delivered huge quantities of reparation bonds and we had before us a good test of the limitations of this method of making payments. These reparation securities could not be sold in foreign markets because Germany was not in a position to pay the interest; they were nonrevenue producing assets and would remain such so long as Germany did not have an export surplus.

We directly discussed the possibility of making larger payments than the amount of the actual export surplus through the delivery of industrial securities —the principal being credited and interest and sinking fund alone being actually paid through the medium of an annual export surplus. But we pointed out that until an export surplus existed, industrial securities would be no better than government reparation bonds. In an unguarded moment Germany might be credited with having made legal payments, but if the real payments were not forthcoming "the Reparation Commission would later have to uncredit Germany's account" (page 20).

Incidentally, Mr. Young took the occasion to express his belief that Germany still possesses a considerable volume of foreign investments. Completely ignoring our detailed discussion in Appendix A and our reasons for taking a figure of 25 billions as the gross and 20 billions as the net amount of German foreign investments in 1914, Mr. Young bases his estimate of 30 billions (apparently *gross* —but no mention is made of the 5 billions offset) chiefly upon a compilation which he made during the war for the War Trade Board, in which it was found

that the total of German holdings in Latin America amounted to from 4 to 8 billion marks. Assuming this estimate to be accurate within the broad limits set, we submit that by itself it throws no light upon the *total* of German foreign investments; at best it is only an evidence of concentration in a certain quarter of the globe. Anyone at all familiar with the German international balance of accounts and with German financial affairs before the war, knows that the rate of increase in German foreign investments was a diminishing one in the later years. This was a matter of common observation and of concern among German economists and German financiers.

Taking his 30 billion estimate as a starting point, Mr. Young concludes that the total of German foreign investments at the present time is probably 10 billions, though he thinks that not more than "four or five billions are liquidatable in the sense that they might be converted into funds for reparation payments." While the review was written before the report of the second Committee of Experts appeared, Mr. Young endeavors in a footnote inserted after the report was available, to make it appear that his figure of 10 billions of foreign investments is as close to that of the Experts' figure as was our estimate of 2 to 3 billions. Now Mr. Young's figure of 10 billions is for securities alone; our figure of 2 to 3 billions was for bank balances abroad, and not investments—a fact not observed by the reviewer—while the Expert Committee's figure of 6.75 billions is for foreign assets of every kind and description. It includes not merely the remnants of pre-war foreign investments, but also the balances that have been built up by the sale of paper marks, real estate, etc., plus the private property of Germans that has been left in ceded territories. Moreover, this 6.75 billions is not a net figure, because there must be subtracted from it at least 2½ billions, a figure (which we are confident is materially understated)[1] which is the committee's estimate of the amount of real estate and securities in Germany owned by foreigners. Now, the committee's report does not try to apportion the 6.75 billions among the three categories, but with reference to foreign investments they take 28 billions as the pre-war total—like Mr. Young, failing to differentiate between gross and net foreign investment. They conclude that at the end of 1923 Germany's "foreign investments represented only a small fraction of the pre-war holdings and that not all of these are productive."

Mr. Young's second contention was that

> The authors hardly attach sufficient importance to the leading rôle played in the progress of German inflation by the unremitting pressure for foreign payments. In their view, inflation was the necessary result of an unbalanced budget. They fail to note that foreign debts are even more potent causes of inflation than are domestic debts, and that the pressure of foreign debts more than anything else made the balancing of the German budget impossible.

A close reading of this not altogether explicit statement indicates that the reviewer apparently has two points in mind: the first is that we largely ignore the influence of foreign debts in causing inflation; and the second is that we regard inflation as a mere result—not a cause—of an unbalanced budget. These charges came as a very distinct surprise to the authors, for they had hitherto been commonly criticised for attaching too much importance to the influence of external

[1] See discussion in Moulton: *The Reparation Plan*. New York: 1924. Chapter III.

obligations upon both currency inflation and the budget. While it is our view that it is a fruitless task to attempt to disentangle and measure the relative influence of interacting forces, we repeatedly emphasized the importance of the disastrous effects of peace-treaty obligations upon both the monetary and the fiscal system of the country.

We first discussed the effects of foreign obligations upon German inflation in Chapter III (What Germany Has Paid and How). On page 86, there is an italicized sentence reading, "*Without the sale of paper currency Germany would have been forced to an early default on cash reparation payments.*" This is followed by the statement that the "German Government, in coöperation with the Reichsbank, naturally resorted to the sale of paper currency whenever it proved impossible to procure in any other way funds with which to meet reparation instalments."

In Chapter V (The Budgetary Problem) we say that "Each purchase of foreign exchange for the purpose of making treaty payments or deliveries was accompanied by a roughly proportionate increase in the 'floating' indebtedness incurred through the placement with the Reichsbank of treasury bills in exchange for the banknotes used in buying foreign exchange and followed by a corresponding decline in the value of the mark" (page 169). "Every political and economic development since May, 1921, relating directly or remotely to the fulfillment of the treaty, has accelerated the increase in the floating indebtedness" (page 172). A long footnote on the bottom of the page stresses the difficulties of the situation in January, 1923.

In Chapter VI (The Monetary Situation), in discussing whether monetary inflation *could* not have been prevented by the sale of domestic bonds, we point out in an italicized sentence on page 207 that "*The raising of funds from domestic bond issues could not create foreign balances.*" We go on to emphasize the point that "Germany was continually under pressure to procure *foreign* currency . . . ; and this could not be procured by domestic bond sales alone. . . . The German Government, therefore, in order to acquire funds with which to meet foreign payments, had no practical alternative to the policy of interposing as few obstacles as possible to the ready replenishment of the supply of currency intended for export." For emphasis, we return to this point on the following page and state: "There was a continued necessity for raising extraordinary amounts of cash in consequence of the necessity of making reparation payments abroad. Once more let it be clearly understood that if bonds could not be sold abroad, paper money had to be sent abroad."

With reference to the charge that we regard inflation as only a result, and not a cause, of an unbalanced budget—it may be noted that in the budget chapter, page 173, we have an italicized sentence which reads: "*The progressive depreciation of the currency demoralized the budget,*" which is followed by a discussion of the reasons therefor. Again, in order that the reader might not be under any misapprehension as to our view that inflation is a cause as well as a result of an unbalanced budget, we insert an italicized sentence in the chapter on The Monetary Situation, on page 211, which reads as follows: "*Rapid inflation completely disorganizes the government budget.*" We add, "Once inflation has gained momen-

tum it becomes utterly impossible to increase taxes and reduce government expenditures and thereby obviate the necessity of issuing additional supplies of paper currency. The budgetary discussion in the preceding chapter has indicated the disastrous effects of the rapid inflation of 1922 upon the German budget."

Space does not permit the presentation of evidence on a third point where Mr. Young has placed our position in an entirely false light. He directly implies that the authors conclude from their study of the pre-war status that Germany cannot possibly develop a favorable balance of trade. We can only say that we drew no such conclusion, direct or implied.

H. G. MOULTON
C. E. MCGUIRE

A REJOINDER

This is more evidence that the reviewing of books is a thankless task. I had hoped that I had done the authors of *Germany's Capacity to Pay* no injustice. I had hoped, furthermore, that my review was courteous and appreciative in tone. From the content and the manner of their reply I gather that in their opinion I achieved a double failure. I cannot see the matter as they do, for I have gone over the review again and find only one point at which I should want to make a substantive change.

1. "While endorsing the volume in the large," they say, I devote the "bulk" of my analysis to "expressing disagreement on two major points." My review covered nearly five pages of this journal. To the first of the two points I gave about a page, to the second seven lines. For the rest, there was a good deal of specific as well as general approval. There were also other criticisms, quite as serious, in my judgment, as those to which Messrs. Moulton and McGuire now reply.

2. I do not understand why Messrs. Moulton and McGuire should protest that in their view the rigid export-surplus doctrine holds true "from year to year," "over a period of years," "in the long run." Such was precisely my understanding of their position. That position, I thought, over-simplified a very complex problem. As they explain above, they attached two different meanings to the word "payment," and associated the possibilities of refunding only with what they call "temporary payments." When I said that they concerned themselves with economic and not with legal *payment* (note that I used the singular), the context makes it clear that I referred to the final payment or *discharge* of a debt not impossibly large. I gave specific illustrations of what I meant by legal as distinguished from economic payment. Those illustrations cover a type of phenomena not discussed or even alluded to by Messrs. Moulton and McGuire. Their present contention, therefore, appears to be without merit.

3. My thesis with respect to the second "major point" was that "foreign debts are *even more potent* causes of inflation than are domestic debts." Messrs. Moulton and McGuire have overlooked the significance of the three words I have now italicized. Their reply shows that I was right when I held that they had not attached sufficient importance to the "leading rôle played . . . by the unremitting pressure for foreign payments." They could not have taken account of

it in their book, for they fail to understand it now. Nevertheless the thesis is by no means novel, and they must have encountered it before. Foreign debts (or attempts to pay such debts) have a *direct* effect upon the *foreign exchanges*, and in that way upon prices, the budget, and the further issue of paper currency. Doubtless, if I had given similar details in the review, my meaning could not have been so completely misunderstood. I supposed that the thesis was familiar and so did not elaborate it.

4. On the question of the amount of Germany's pre-war foreign holdings, I did not "ignore" the authors' detailed discussion in their Appendix A. I gave it careful consideration and specifically referred to it. My own estimate was not "based chiefly" upon the inventory of German holdings in Latin America (with the making of which I should not be credited). It was, in fact, a slight modification (upward—from 28 billions to 30 billions) of an estimate I made in 1919 (with able assistance) for the American Commission to Negotiate Peace. It is only just to my associates of 1919 to say that the methods by which the estimate was put together were not as stupid as Messrs. Moulton and McGuire imply.

5. My figure of ten billions as the amount of German foreign holdings in 1923 was *not* for securities alone, and nothing that I said warrants the inference that it was. If anything, my estimate was more rather than less inclusive than the McKenna estimate.

6. I erred in not specifying that the authors' estimate of Germany's foreign holdings in 1923 covered only balances in banks. I was misled by their statement (page 81), "The liquidation of former German investments abroad is practically complete." Elsewhere (page 289) they explain that it was the "net remnant" which they thought had "dwindled to practically nothing." On a gross basis, I suppose, their figure would be notably closer to the McKenna estimate than my own estimate was.

7. Messrs. Moulton and McGuire complain that I "directly imply" that they conclude "that Germany can not possibly develop a favorable balance of trade." What I said was: "The outlook for a possible favorable balance of commodity trade is not quite so hopeless as the authors make it appear." The question might be dismissed as hinging only upon the nuances of the English language. But it happens that the sentence merely summarizes the conclusions of a paragraph in which my specifications are set forth too precisely to mislead either Messrs. Moulton and McGuire or any other readers.

ALLYN A. YOUNG

Harvard University

PROGRESS OF WORK IN THE CENSUS BUREAU

CENSUS OF AGRICULTURE

An office force of more than 1,000 is now engaged in compiling the returns of the census of agriculture. The great part of this force consists of clerks and punching machine operators appointed within the last few months. Of the nearly 6 million farm schedules which have been received and counted, over

2½ million have been edited and made ready for the card punching. The schedules are now being edited at the rate of over 50,000 a day.

As the field work approached completion it was found that the amount of the $3,500,000 appropriation available for the office work, would not be sufficient for carrying out the original plan of tabulation, which involved the use of eight different forms of punch cards. It was, therefore, decided to omit from the first tabulation three cards representing 34 items on the schedule, or approximately one-third of the total cards to be punched. The items omitted from the primary tabulation are: Value of implements and machinery, farm debt, farm expenses, coöperation in marketing, tractors, radio, farm population, sheep and goats, wool and mohair, animals slaughtered, and poultry.

As rapidly as the tabulation is completed for a county some of the principal items are published in the form of a preliminary press announcement, as follows: Number of farms, by tenure (and by color of operator in Southern States); farm acreage, in detail as to crop land and pasture; farm values, with land and buildings separately; livestock on farms; and acreage and production of principal crops. Comparative figures of the 1920 Census are also shown. These announcements have been issued for 175 counties, with totals for two states—Rhode Island and Nevada.

CENSUS OF MANUFACTURES, 1923

The results of the 1923 census of manufactures are being published in a series of 60 or more bulletins covering the principal manufacturing industries of the country. Of these, three, relating respectively to the manufacture and sale of farm equipment, pulp-wood consumption and wood-pulp production, and cross-ties and poles purchased have already been issued, and 23 in manuscript or proof are in the hands of the printer.

In addition to these bulletins, preliminary reports for each state presenting data for its leading manufactures, are now being issued at short intervals, 33 having been given out up to date.

An arrangement has been made by which the Chambers of Commerce in those cities in which they assisted the Bureau in the task of taking the census will be supplied with advance figures for publication covering the industries of the particular city. Statistics for 20 cities have thus far been published through this arrangement.

FINANCIAL STATISTICS OF CITIES AND STATES, 1923

The 1923 reports on the financial statistics of states and cities will contain a complete and detailed classification of revenues and expenditures as recommended by the Census Advisory Committee, and will be the first reports to give full detail since that for the year 1919, no report having been issued for 1920 and those for 1921 and 1922 giving a condensed or summary classification. Copy for the 1923 state report was sent to the printer in March, and that for the 1923 city report in April.

Press summaries have been given out for each state and each of the 248 cities having a population of over 30,000. In the meantime two bulletins have been

printed, one giving the principal items for all states and the other for all cities. It is expected that these bulletins will in a large measure satisfy the demand for the state and city statistics and thereby obviate the necessity for a large edition of the final reports.

CENSUS OF INSTITUTIONAL CLASSES, 1923

The report on Hospitals and Dispensaries and that on Paupers in Almshouses have been published, and that on Patients in Hospitals for Mental Disease has gone forward to the Public Printer, so that of the reports covered by the recent decennial census of institutional classes there now remain three not yet completed, comprising those on feeble-minded and epileptics in institutions, children under institutional care, and prisoners.

MARRIAGE AND DIVORCE

The report on Marriage and Divorce for 1922, giving the results of the first compilation of statistics on these subjects since 1916, was issued in January. This has now been followed by the 1923 report published in April. In the meantime the 1924 data are being collected, so the prospect seems favorable for the regular annual compilation of these statistics hereafter.

VITAL STATISTICS—EXTENSION OF THE REGISTRATION AREA

Of the 48 states 39 are now in the death registration area and 33 in the birth registration area, these two areas comprising respectively 88.4 per cent and 76.1 per cent of the population of the United States. Encouraging progress is being made in the "drive," which has as its slogan "every state in the Registration Area before 1930." That means that 9 states must be added to the death registration area and 15 to the birth registration area within the next five years. A recent telegram from Arizona announces the enactment of a good vital statistics law in that state. West Virginia and Alabama are now being tested for admission to the areas, and Arkansas has asked for a test. So the drive is on.

With a view to improving the accuracy of the vital statistics, the field agents of the Bureau, who may have occasion, in connection with other matters, to visit or pass through a state capital have been instructed to go to the office of the state registrar and check the work of the transcribers who make the copies of the original birth and death certificates to send to Washington.

REPORTING AUTOMOBILE FATALITIES

Because of the great interest attaching to statistics of deaths from automobile accidents, the number of which is increasing at an alarming rate, arrangements have been made to have reports from cities of 100,000 population or more sent in to this Bureau every week by telegraph for publication in a flier called Automobile Fatalities; and for use in the regular annual reports the local registrars are requested to record the place where the accident occurred, which in many instances, of course, is not the same as the place where the death occurred.

J. A. H.

MISCELLANEOUS NOTES

Social Science Research Council Fellowship Appointments. — The Social Science Research Council at its annual meeting in Chicago on April 4th announced the appointment of the following fifteen scholars as research fellows of the Council for the year 1925-26, selected from a total of 108 applicants:

Luther Lee Bernard, Ph.D., Professor of Sociology, University of Minnesota. Problem: A study of the development of the social sciences in Argentina with special reference to the economic, political, and other cultural circumstances under which they were developed.

Charles Warren Everett, M.A., Instructor in English and Comparative Literature, Columbia University. Problem: Life of Jeremy Bentham and the editing of his unpublished manuscripts.

Harold F. Gosnell, Ph.D., Instructor in Political Science, University of Chicago. Problem: Factors determining the extent of Popular Participation in Elections in Typical European States.

Marcus Lee Hansen, Ph.D., Assistant Professor of History, Smith College. Problem: A basic study of the origins of the foreign elements in the settlement of the Upper Mississippi Valley.

Joseph Pratt Harris, Ph.D., Instructor in Political Science, University of Wisconsin. Problem: Workings of election registration systems in the United States.

William Jaffee, Docteur en Droit; Tutor in French and Economics, College of the City of New York. Problem: The Industrial Revolution in France.

Edgar W. Knight, Ph.D., Professor of Education, University of North Carolina. Problem: A study of the Folk High Schools in Scandinavian Countries, especially Denmark, Sweden, and Finland.

Simon S. Kuznets, M.A. (Candidate for Ph.D., June, 1925, Columbia); Fellow in Economics, Columbia University. Problem: Secular Trends in Economic Theory, their interrelations and their bearing upon cyclical fluctuations.

Rose S. Malmud, M.A., Graduate Student, Columbia University. Problem: The Psychology of Literary Ability.

Thomas P. Martin, Ph.D., Associate Professor of American History, University of Texas. Problem: A study of Anglo-American Relations as influenced by economic, political and social forces playing within and between the two peoples.

Hutzel Metzger, M.S. (Candidate for Ph.D., June, 1925, University of Minnesota); Part-time Research Assistant, University of Minnesota. Problem: An Analysis of the Price of Certain Farm Products, with a view to deriving information that will promote the better adjustment of agricultural production.

Ernest E. Mowrer, Ph.D., Assistant Professor of Sociology, Ohio Wesleyan University. Problem: Family Disorganization as a Socially Inherited Behavior Pattern.

Mildred Dennett Mudgett, Ph.D., Assistant Professor of Sociology, University of Minnesota. Problem: Legislation Affecting the Pre-School Child in Certain European Countries.

Sterling Denhard Spero, Ph.D., Fellow, New School for Social Research. Problem: The Position of the Negro in Industry.

Dorothy Swaine Thomas, Ph.D., Research Assistant, Federal Reserve Bank of New York. Problem: The Economic Factor in Crime.

The fellows will travel and study in the following countries: Argentina, England, Belgium, Germany, France, Italy, and Scandinavian countries of Denmark, Sweden, and Finland.

These are the first awards of the Social Science Research Council. Plans have been made to offer research fellowships annually for the following four years.

The following are the officers of the Council: President, Dr. Charles E. Merriam, University of Chicago; Vice-President, Dr. John R. Commons, University of Wisconsin; Secretary, Dr. Horace Secrist, Northwestern University; Treasurer, Dean E. E. Day, University of Michigan. The Council consists of twenty-one delegates elected three from each of the following national scientific societies: The American Economic Association, The American Political Science Association, The American Statistical Association, The American Sociological Society, The American Anthropological Association, The American Historical Association, and The American Psychological Association.

The Committee on Research Fellowships of the Council consists of Dr. Wesley C. Mitchell, Chairman, Professor of Economics, Columbia University; Dr. Charles E. Merriam, Professor of Political Science, University of Chicago; and Dr. F. Stuart Chapin, Secretary, Professor of Sociology, University of Minnesota.

Handbook of Instructions for Standardizing Tables.—A committee of the Division of Statistical and Historical Research of the Bureau of Agricultural Economics, of which Division Dr. O. C. Stine is chief, has prepared a *Handbook of Instructions for Standardizing Tables* (March, 1925) which perhaps will be found of interest to statistical workers outside of the U. S. Department of Agriculture. The Handbook (which we hope will be put in printed rather than mimeographed form later on) gives a very comprehensive list of instructions governing manuscripts, fitting tables and text to the printed page, size of paper and margins, titles, ruling, stubs and box headings, totals and averages, description of years, units, footnotes, sources, index numbers, etc. The instructions, of course, were prepared to meet the needs of the Division in question, but it would seem that many of them might be applied in statistical work generally. Examples of instructions, picked at random, are as follows:

"When practical begin title with keyword or phrase followed by a colon. In a cost of production bulletin a title might read, Potatoes: Cost of Production in Ohio, 1910–1920.

When in a table of figures an average percentage is computed it should be computed from the averages of the totals *not* from the total of the percentages.

When an average is not a simple arithmetic average of figures appearing in the table, the nature of the average should be stated.

Use care in designating years. Use 'year' to designate calendar years. For any other 12-month period use "year beginning . . . " where practical.

In source citations use "rearranged" in the sense of putting unchanged data in a different tabular form. Use "compiled" in the sense of collecting specific data from various sources and assembling in tabular form or making necessary computations and assembling in tabular form with preparation for computations or additional work."

The Handbook should prove a valuable aid to statistical workers in the Department of Agriculture. In standardizing the forms of statistical presentation it should help to reduce printing costs and aid in the wider dissemination of the information contained in the department's statistical reports.

Study of Hog Prices.—G. C. Haas and M. J. B. Ezekiel of the Bureau of Agricultural Economics of the U. S. Department of Agriculture have recently completed a preliminary report on "What Makes Hog Prices", a study of prices and production over 20 years to determine what factors influence the price. The results of this study may be briefly summarized as follows:

1. Besides the supply of hogs brought to market, the value of the dollar and the level of demand are factors determining hog prices at any given time.

2. The level of the demand for hogs varies from time to time with changes in export demand, in the price of steers (and hence of beef), in the quantity of hog products in storage, and with changes in business conditions. The demand was tending steadily upward before the war. After the war period demand dropped to a lower level than before the war with some subsequent recovery.

3. The supply of hogs varies as farmers respond to differences in the relation between corn prices and hog prices. It has usually taken about a year to a year and a half before changes in breeding began to affect market receipts and prices.

4. By estimating future supplies from the relation between present corn and hog prices and forthcoming supplies, and estimating the future trend of business conditions from the prices of industrial stocks, hog prices before the war could have been forecasted six months in advance with a high degree of accuracy.

5. The present situation (in March, 1925), indicates that hog prices will be rising during 1925, and will remain high until at least the spring of 1926.

Estimating the Population of the United States.—Preliminary figures recently announced by the National Bureau of Economic Research, Inc., estimated the population of the United States on January 1, 1925, at 114,311,000. This estimate was made by Willford I. King, Economist of the Bureau, on the basis of the births and deaths in the registration area, as reported by the Census, and the net migration as shown by the records of the Commissioner of Immigration. Adjustments have also been made to cover the supposed under-reporting both of births and of the number of immigrants. The extent of such under-reporting has been estimated from the discrepancy between population estimates in 1920 obtained from records of immigration, births, and deaths since 1910, as compared with the actual Census of 1920.

Figures obtained in this manner indicate that four-fifths of the increase in population in 1924 was due to excess of births over deaths, while in 1923 the natural increase was responsible for only about five-eighths of the entire gain. The difference was due to the fact that the net immigration in 1924 was less than one-half as much as in 1923. The falling off was probably due partly to the less active state of business, as well as to the more stringent restrictions on immigration. The Bureau estimates that in the last sixteen years net immigration into the United States has amounted to 5,624,000, while the excess of births over deaths has totalled 19,330,000.

The trend of the death rate has been distinctly downward during the last sixteen years, as is shown by the fact that the lowest death rate in the first four years of the period was 13.9, while the highest death rate in the last four years of the period was 12.2 per thousand population. The birth rate is also apparently on the decline, the lowest birth rate between 1909 and 1911 being 25.3, while the highest birth rate between 1922 and 1924 was 23.4. The estimates for the years since 1919 appear in the following table:

Year	Population at close of year (thousands)	Per cent gain in population during year	Net immigration (thousands)	Births (thousands)	Deaths (thousands)
1919.....	105,711	1.14	−13	2,552	1,352
1920.....	107,412	1.61	446	2,645	1,390
1921.....	109,135	1.60	231	2,748	1,256
1922.....	110,688	1.42	278	2,567	1,292
1923.....	112,684*	1.80*	747	2,606*	1,357*
1924.....	114,311*	1.44*	315	2,645*	1,333*

* Preliminary estimates.

An Analysis of State Automobile Fatality Records.—What effect has the drivers' license law had upon the trend of fatal automobile accidents in the states having such laws? How has this trend been affected by the increase in motor vehicle registration? What other factors enter into an analysis of this subject and to what extent?

In a recent article appearing in the *National Safety News*, published by the National Safety Council, Chicago, an attempt was made to analyze these fatality records. The interpretation and the inferences drawn were presented tentatively, in the hope that they would at least arouse further discussion.

An index figure was developed to give cognizance to the increase in motor vehicle registration, the increase in population and the area of the state. This "fatality index" is intended as a "yard stick" by which one state may be compared with another and by which the efficacy of accident prevention work in any state may be determined. The formula is:

$$F = \frac{D}{P} \div \sqrt{\frac{R}{A}}$$

Where F = fatality index
D = automobile fatalities
R = automobile registration
A = land area in square miles

The basis for using the square root of the automobile density is found in certain studies by city planning and traffic experts indicating that in a given community at different times the number of automobiles actually on the street tends to vary in proportion to the square root of the number registered.

Curves of the death rate (deaths per 100,000 population) and the fatality index were plotted on logarithmic paper to show the percentage increase or decrease by years, from 1916 to 1923 inclusive. The fatality index was averaged and compared by periods. The average of the first four years (1916–1919) was compared with the second four years (1920–1923) and with the last two years (1922–1923), and the states were listed in order of the percentages of the average of the last two years with the average of the first four.

It was found that most of the states having drivers' license laws, especially where such laws are efficiently administered, fell in the group of low percentages. Among the states having low percentages, other than drivers' license law states, several had local safety councils in the large cities forming a large proportion of the population and motor vehicle registration of the state.

The analysis is open for further development. The National Safety Council invites the members of the Statistical Association to develop the study further and will wel-

come any suggestions or criticisms in their attempt to determine what factors influence these state records. A copy of the analysis as it appeared in the February issue of the *National Safety News* will be sent to any member of the Statistical Association upon application to the National Safety Council, Chicago.

Meeting of Washington Members of the American Statistical Association.—Washington statisticians held a very successful dinner meeting at the Grace Dodge Hotel on the evening of March 20th. Seventy-two local members of the American Statistical Association and their friends attended this dinner in response to a printed announcement mailed out by a committee consisting of Messrs. William M. Steuart, E. Dana Durand, E. A. Goldenweiser, J. H. Parmelee, Joseph A. Hill, John Cummings, O. C. Stine, Lewis Meriam, M. O. Lorenz and Dorsey W. Hyde, Jr. Dr. Joseph A. Hill acted as chairman.

The meeting was called for two purposes: first, to hear an address by Dr. Willford I. King, Secretary of the American Statistical Association, who kindly consented to visit Washington in response to an invitation from the local committee, and second, to consider a suggestion "that Washington statistical workers form a local body for the purpose of holding regular meetings and conducting such committee work as may be helpful in furthering, locally, the objectives in which statisticians are peculiarly interested."

Dr. King discussed the present status of statistical work as a profession. He pointed out certain mistakes in theory and in methods, and made a strong plea for more imagination and foresight in the planning and coördination of statistical undertakings. He then emphasized the importance of organized professional effort in furthering the use and general effectiveness of statistical work. He pointed out that the American Statistical Association has been engaged since 1839 in furthering the progress of statistics and that, as a result of such efforts, the use of statistics has become general and statistical science is making important contributions to the national welfare. In closing he stressed the value of local organization, pointing to recent developments in Philadelphia, Cleveland and other cities, and he wished all success to Washington statisticians in any plans they might develop to further statistical interests in the national capital.

At the conclusion of Dr. King's address Dr. Hill called attention to the proposal that Washington statisticians form a local body, and he then asked Dr. E. Dana Durand to present this proposal in more detail. Dr. Durand spoke very interestingly upon the valuable service which such an organization might render. He cited specific instances where greater coöperation might result in an increase in the value of statistical data compiled by various federal departments and agencies.

Dr. Hill then called upon the members present for a general discussion. Dr. E.A. Goldenweiser responded with a short talk in which he referred to various local efforts to develop training courses for statistical workers, and he pointed out that a local association could render helpful service by working for the coördination and standardization of such training work. Mr. Lewis Meriam followed with a statement on the important work a local body could do in compiling class specifications for statistical workers in the federal service in connection with the application of the Classification Act of 1923.

A motion was then presented that a committee be appointed, including the acting chairman, to draw up by-laws for a local branch of the American Statistical Association, and that this committee be authorized to call a second meeting during the spring. This motion was duly passed and the following members were appointed for this task: Messrs. Joseph A. Hill, E. Dana Durand, E. A. Goldenweiser and Lewis Meriam.

Philadelphia Statistical Dinner.—A group of Philadelphia members of the American Statistical Association met at dinner at the City Club on March 13. Dr. I. M. Rubinow of the Jewish Welfare Society presented a paper on "The Relation Between Social and Business Statistics." He emphasized the bond of a common technique, and suggested that the business statisticians have more need of contact with social statistics than the social statisticians have with business statistics, since the business statistician is concentrating on a narrower field, and is likely to fail to realize the bearing of certain broader variables upon his interests.

Professor Lincoln W. Hall of the Wharton School of Finance and Commerce, in discussing Dr. Rubinow's paper, took the stand that business and social statistics are independent disciplines, and have no necessary common point.

Miss Anna Bezanson of the Department of Industrial Research, University of Pennsylvania, presented the results of a survey which her department is bringing to completion on the subject of "Labor Turnover in Relation to Business Cycles." The investigation indicates that the business cycle has been, during the past two years, the important factor determining fluctuations in separations from service, and that this is true of resignations as well as of discharges. Differences between establishments, however, are maintained fairly constantly through the cycle.

That the proportion of Domestic Relations Court cases avowedly involving drunkenness dropped very markedly upon the initiation of prohibition, and has since that time climbed slowly back, until it has now reached pre-prohibition levels, was the conclusion of a paper presented by Mr. Frank S. Drown, Statistician of the Philadelphia Municipal Court.

Logical considerations involved in the interpretation of differences between percentages were discussed in a paper on "The Statistical Search for Causes and Symptoms," by Dr. Hornell Hart of Bryn Mawr College.

Meeting of Cleveland Chapter.—A dinner meeting of the Cleveland Chapter of the American Statistical Association was held on Thursday, March 12. The discussion was led by James F. Hughes. There were seventy-four people present. Papers were given by the following persons:

Dr. Leonard P. Ayres, Cleveland Trust Co., "The Controlling Factor in Security Price Movements."

John W. Hill, Financial Editor, *Iron Trade Review*, "Statistics the Business Man Wants."

Mrs. Wm. E. Beckwith, Western Reserve University, "The Statistical Measurement of Distribution of Income."

Harmen B. Flinkers, Federal Reserve Bank, "Time Lags in Cyclical Changes in Retail and Wholesale Trade."

John Love, Financial Editor, *Plain Dealer*, "The Element of Chance in Business Success."

The United States Bureau of Labor Statistics has recast its wage data in certain industries employing both men and women, for the light that it may throw upon the question as to whether the employment of women is increasing relatively more rapidly than that of men in the more established lines of industry. The industries discussed are cotton mills, woolen and worsted mills, men's clothing industry, hosiery and underwear, and the boot and shoe industry. Selected occupations are shown for the industries for a period 1907 to 1913, inclusive, and then for the entire industry from 1914 to 1924, inclusive. The facts shown are the number of men and number of women, the

percentage that women form of the total, earnings per hour for men and women, and the percentage that women's earnings form of the men's earnings. Commissioner Stewart presents the figures in a special article in the April *Monthly Labor Review*.

Field work has been begun by the Bureau of Labor Statistics on wages and labor conditions in sawmills and logging camps, foundries and machine shops, and in potteries.

A very interesting table on "Trend of Occupations in the Population" appears in the May, 1925, *Monthly Labor Review*. Going back to 1850, an attempt is made from the Census material to show the number of carpenters, etc., etc., per million of population, keeping the classification down to trades as much as possible, but where that is impossible, then an industry such as the employees in blast furnaces, for instance.

The Statistical Committee of the National Safety Council, under the chairmanship of Dr. L. I. Dublin, Chief Statistician of the Metropolitan Life Insurance Company, has developed a system for reporting accidents which has already been adopted by many cities and which is the basis of new forms proposed for the entire state of New York. This is a worthy attempt to gain more accurate, reliable, and comprehensive information regarding the occurrence of accidents. Samples of these forms will be sent to interested persons upon application to the National Safety Council, 168 North Michigan Avenue, Chicago, Illinois.

Mr. Carl Snyder of the Federal Reserve Bank of New York will give a course of thirty lectures in the Columbia University Summer School on "Business Measurements and Business Cycles." The course will be divided into six sections: Foundations, Historical Survey, Forces of the Business Cycle, Theories and Realities, Predictability and Methods of Forecasting, and Statistical Control of Business.

Mr. E. C. Rhodes, Reader in Statistics, University of London, London School of Economics and Political Science, will offer two courses in statistics at Northwestern University during the Summer session, June 22 to August 15, 1925. Mr. Rhodes took the Mathematical Tripos with honors at Cambridge, and the degree M.Sc. at the University of London. He was with Karl Pearson at University College for three years, and lecturer in mathematics at University College, Leicester, for two years. He is a frequent contributor to *Biometrika* and other statistical journals.

The courses offered are (1) "An Introductory Course in Statistical Methods," and (2) "Advanced Mathematical Statistics," presupposing a knowledge of differential calculus.

The Macmillan Company, New York, has in press an entirely rewritten and enlarged edition of Professor Horace Secrist's book, *An Introduction to Statistical Methods*.

Professor Horace Secrist, who was on leave of absence from Northwestern University during the first semester of this year, spent some time in England at the Universities of London, Oxford, Cambridge, Manchester, and Liverpool studying the contents of courses and methods of instruction in statistics and economics.

Professor W.F. Ogburn expects to be in Europe during the summer and fall. In his absence the editing of the JOURNAL will be in the hands of Associate Editor Professor Frank Alexander Ross, of Columbia University.

MEMBERS ELECTED SINCE MARCH, 1925.

Abbott, Roy T., The Shelton Looms, 395 Fourth Ave., New York City.
Adams, William W., U. S. Bureau of Mines, Washington, D. C.
Andrews, Benjamin R., Teachers College, Columbia University, New York City.
Archibald, Lester E., Division of Statistics, State House, Boston, Mass.
Atkinson, Robert K., Russell Sage Foundation, 130 E. 22nd St., New York City.
Auld, George P., Haskins & Sells, 30 Broad St., New York City.
Babson, Paul T., The United Business Service, 210 Newbury St., Boston, Mass.
Bachelor, Robert W., Commercial Dept., Pacific Telephone & Telegraph Co., San Francisco, Cal.
Bailey, Herbert P., Statistician, The Warner & Swasey Co., 5701 Carnegie Ave., Cleveland, Ohio.
Baron, Albert H., American Public Utilities Bureau, 50 Madison Ave., New York City.
Bates, Sanford, Dept. of Correction, State House, Boston, Mass.
Beam, Louis H., Bureau of Agricultural Economics, U. S. Dept. of Agriculture, Washington, D. C.
Bedinger, George R., Public Charities Association of Pennsylvania, 311 S. Juniper St., Philadelphia, Pa.
Benson, Glion T., National Bureau of Economic Research, 474 W. 24th St., New York City.
Bernstein, Lester, Commercial Development Dept., Philadelphia Company, Pittsburgh, Pa.
Bernthal, Walter G., Bureau of Business Research, New York University, New York City.
Bigelow, Horace R., Brookmire Economic Service, 66 Bank St., New York City.
Blakemore, Maurice N., 347 Madison Ave., New York City.
Blyth, Major James, Chief, Statistics Branch, War Dept., General Staff, Washington, D. C.
Bracy, William L., Bureau of Business and Governmental Research, University of Colorado, Boulder, Col.
Bratney, John F., American Telephone & Telegraph Co., 195 Broadway, New York City.
Brook, John, Tidewater Oil Co., 11 Broadway, New York City.
Broun, B. R., New York Telephone Company, New York City.
Brown, Mary Lucile, Washburn-Crosby Co., Minneapolis, Minn.
Byrant, J. E. G., Penn Mutual Life Insurance Co., Philadelphia, Pa.
Buell, Bradley, Secretary, The Council on Immigrant Education, 244 Madison Ave., New York City.
Carmichael, F. L., School of Business, Columbia University, New York City.
Carson, Mary A., Woodlawn, Pittsfield, Mass.
Carter, Leyton E., Director, Municipal Research Bureau of Cleveland, Cleveland, Ohio.
Cartwright, Clermont, c/o Shields & Co., 27 Pine St., New York City.
Chassee, L. J., School of Business, Columbia University, New York City.
Clemen, Rudolf A., Illinois Merchants Trust Co., 230 S. LaSalle St., Chicago, Ill.
Comer, Harry D., Port of New York Authority, 11 Broadway, New York City.
Cook, Esten H., J. B. Geijsbeek & Co., Denver, Col.
Copeland, Lucille, Children's Hospital, Randall's Island, New York.
Coyle, Eunice S., Harvard Economic Service, Cambridge, Mass.

Craig, Cecil R., Lund, Sweden.
Crane, Roy E., American Telephone & Telegraph Co., 195 Broadway, New York City.
Cremer, George S., The Brooklyn Union Gas Co., 176 Remsen St., Brooklyn, N. Y.
Crickman, Chlorus W., Dept. of Agricultural Economics, Iowa State College, Ames, Iowa.
Davis, Dorothy S., Committee on Dispensary Development, 17 W. 43rd St., New York City.
Dennison, Jackson B., Miami University, Oxford, Ohio.
Dinsmore, Wayne, Secretary, Horse Association of America, Union Stock Yards, Chicago, Ill.
Donald, William John, American Management Association, 20 Vesey St., New York City.
Douglas, James R., Security Trust & Savings Bank, 5th and Spring Sts., Los Angeles, Cal.
Fletcher, Vivian A., Western Electric Co., 195 Broadway, New York City.
Flynn, Benedict D., The Travelers' Insurance Co., Hartford, Conn.
Forney, Reuben L., Northwestern University, Evanston, Ill.
French, Earl R., Bureau of Agricultural Economics, 11 Broadway, New York City.
Gardner, Anson B., Westinghouse Electric & Mfg. Co., 150 Broadway, New York City.
Gentry, Robert E., The George Tritch Hardware Co., Denver, Col.
George, Roland J., National City Bank, 55 Wall St., New York City.
Gillett, Roy Lewis, U. S. Dept. of Agriculture and New York State Dept. of Farms & Markets, Albany, N. Y.
Gillman, J. M., University of Pittsburgh, Pittsburgh, Pa.
Grace, G. Vincent, Jacoby, Grace & Co., 46 Cedar St., New York City.
Graicunas, Vitautas A., McGraw-Hill Co., New York City.
Greider, Eugene, Rutgers University, New Brunswick, N. J.
Guerrero, Jose, International Health Board, San Jose, Costa Rica, Central America.
Haake, Alfred P., MacManus Company, 82 E. Hancock Ave., Detroit, Mich.
Hall, Fred S., Russell Sage Foundation, 130 E. 22nd St., New York City.
Hanselman, John J., American Telephone & Telegraph Co., 195 Broadway, New York City.
Hardy, Charles O., Institute of Economics, 26 Jackson Pl., Washington, D. C.
Hervey, George W., Agricultural Station, New Brunswick, N. J.
Hess, Ralph H., Dept. of Industrial Education, Institute of American Meat Packers, Chicago, Ill.
Hill, Miss Alice M., National Tuberculosis Association, 370 Seventh Ave., New York City.
Hill, George B., Box 67, P. O. Station F, New York City.
Hofsoos, Emil, The MacManus Co., 82 E. Hancock St., Detroit, Mich.
Horner, F. C., 224 W. 57th St., New York City.
Jackman, J. Richard, The Rumford Press, Concord, N. H.
Jordan, Robert, Yale School of Medicine, New Haven, Conn.
Jordan, Virgil, National Industrial Conference Board, 247 Park Ave., New York City.
Jeavons, W. R., 2505 Arlington Rd, Cleveland, Ohio.
Jones, James B., c/o Remick, Hodges & Co., 14 Wall St., New York City.
Kimball, Milo, Dept. of Economics, Boston University, Boston, Mass.
Klemmedson, Gunnar S., Dept. of Economics, Colorado Agricultural College, Fort Collins, Col.

Krueger, L. B., Oberlin College, Oberlin, Ohio.
Kyser, Leah. A., Ohio State University, Columbus, Ohio.
Laugh, William H., Business Training Corporation, 347 Madison Ave., New York City.
Lee, Frank H., Jr., The Maccabees, 5065 Woodward Ave., Detroit, Mich.
Leslie, Frank E., Gorgas Memorial Institute, 598 Madison Ave., New York City.
Livermore, Shaw, Harvard Graduate School of Business Administration, Cambridge, Mass.
MacMillan, Mrs. P., Grain Exchanges, Munsey Bldg., Washington, D. C.
Marks, Herbert H., Metropolitan Life Ins. Co., New York City.
McKay, Evelyn C., Milbank Health Demonstration, 105 E. 22nd St., New York City.
Mead, Bennet, U. S. Census Bureau, Washington, D. C.
Meyerson, Hyman, Workmen's Compensation Insurance, State Insurance Fund, 432 Fourth Ave., New York City.
Milton, Ormand, Case, Pomeroy & Co., 60 Beaver St., New York City.
Mims, Mr. James Luther, Southern Union Life Ins. Co., Fort Worth, Texas.
Mort, Paul R., Teachers College, Columbia University, New York City.
Morton, R. L., Ohio State University, Athens, Ohio.
Nason, Miss Celeste, National Bureau of Economic Research, New York City.
Near, Arcadia D., Institute of Economics, 26 Jackson Pl., N. W., Washington, D. C.
Nelson, J. L., Tanners' Council of America, 41 Park Row, New York City.
Newton, George A., Curtis, Fosdick & Belknap, 233 Broadway, New York City.
Nichols, Harold S., Dennison Mfg. Co., Framingham, Mass.
Niederlitz, Joseph G., 630 Gas & Elec. Bldg., Denver, Col.
Nielsen, A. E., 523 W. 123rd St., New York City.
Olson, Lyle H., The American Appraisal Co., 120 Broadway, New York City.
Parry, Carl E., Division of Research and Statistics, Federal Reserve Board, Washington, D. C.
Pawley, Francis R., Producers Distributing Corp., 469 Fifth Ave., New York City.
Payne, Charles K., Washington Square College, New York University, New York City.
Payne, Thomas P., The Brooklyn Union Gas Co., 176 Remsen St., Brooklyn, N. Y.
Pearl, William A., Baker, Kellogg & Co., 120 Broadway, New York City.
Perry, Clarence A., Russell Sage Foundation, 130 E. 22nd St., New York City.
Phillips, J. D. M., National Association of Steel Furniture Mfgrs., 1227 Engineers Bldg., Cleveland, Ohio.
Pinney, Sydney, The Travelers' Insurance Co., 700 Main St., Hartford, Conn.
Powell, Leona Margaret, N. Y. Employing Printers' Association, Room 1710, 461 Eighth Ave., New York City.
Rahde, J. W., Western Union Telegraph Co., 195 Broadway, New York City.
Rice, Chester L., Iron Age Publishing Co., 521 W. 112th St., New York City.
Richards, H. I., Bureau of Agricultural Economics, Division of Farm Management, Washington, D. C.
Richards, Harry S., Cleveland Association for Criminal Justice, Room 705, Federal Reserve Bank Bldg., Cleveland, Ohio.
Sachs, Alexander, 50 Pine St., New York City.
Sanders, Vergil A., New England Crop Reporting Service, Wakefield, Mass.
Scott, H. C., Riordon Sales Corporation, Ltd., 367 Beaver Hall Square, Montreal, Canada.
Scott, Richard M., Denver Chamber of Commerce, Denver, Col.
Sheehan, Irene C., Baker, Kellogg & Co., 120 Broadway, New York City.

Slade, Helen, 15 W. 55th St., New York City.
Smelser, David Paul, Procter & Gamble Co., Cincinnati, Ohio.
Smith, Francis F., University of California, Berkeley, Cal.
Smith, John A., 67 Wall St., New York City.
Stevens, Earnest V., Central Station, Elec. Light & Power, Room 1539, 72 W. Adams St., Chicago, Ill.
Stevens, Chester D., U. S. Dept. of Agriculture, Box B, Wakefield, Mass.
Sowers, Don C., University of Colorado, Extension Division, Boulder, Col.
Tammany, S. B., The Krebs Pigment & Chemical Co., Newport, Del.
Taylor, James S., University of Pittsburgh, Dept. of Mathematics, Pittsburgh, Pa.
Tewksbury, Russell B., Graduate Student, Johns Hopkins University, Baltimore, Md.
Thompson, William J., The Colorado & Utah Coal Co., 1203 First National Bank Bldg., Denver, Col.
Theis, Sophie van Senden, State Charities Aid Association, 105 E. 22nd St., New York City.
Tilton, Thaddeus F., Cambridge Associates, 174 Newberry St., Boston, Mass.
Trimble, James R., Mutual Benefit Life Ins. Co., Newark, N. J.
Towne, Richard P., National Blank Book Co., Holyoke, Mass.
Villamin, Vicente, 4966 Broadway, New York City.
Watkins, Myron W., National Industrial Conference Board, 247 Park Ave., New York City.
Weaver, F. P., Dept. of Agricultural Economics & Farm Management, Ithaca, N. Y.
Weil, Charles A., Alphonse Weil & Bros., 100 Gold St., New York City.
Westerfield, Prof. Ray B., Yale University, New Haven, Conn.
Whelden, Chester H., Jr., Yale University, New Haven, Conn.
Whitney, Caroline, Columbia University, New York City.
Wightman, Earl C., Detroit Life Insurance Co., Detroit, Mich.

REVIEWS

Unemployment Insurance in Great Britain: A Study in State Socialism, by Felix Morley. (Hart-Schaffner & Marx Prize Essays. Volume XXXVIII.) Boston: Houghton-Mifflin Co. 1924. xvi, 204 pp.

This study of the experience of Unemployment Insurance comes well recommended as a prize essay of the well-known Chicago series. It comes at an opportune time in view of the initiation of very important experiments in unemployment insurance in the Men's Clothing Industry of Chicago, New York, and other centers, as well as the propaganda for unemployment insurance legislation in several states.

The author is to be congratulated upon the skill with which he succeeded in embodying in a very short compass of some 200 pages, or only 176 pages when the appendices are deducted, the story of a very confusing avalanche of legislation and a statistical review of the results of such legislation. The style is concise and precise, but nevertheless lucid. Finally, it may be safely assumed that the story is accurate. The very fact of the award of the prize by a committee of most prominent economists is sufficient guarantee. A very careful reading of the book may, therefore, be recommended to anyone interested in the subject, who has not had the opportunity of following the numerous changes introduced in the system under the pressure of post war conditions.

But Mr. Morley endeavors to present much more than an objective recital of figures and events. It presents a very severe arraignment of the whole structure of State Employment Insurance in Great Britain, with an equally definite leaning towards a substitute in the nature of what has become known as "unemployment insurance by industry." Because of the importance of this problem a critical review of Mr. Morley's criticisms may not be amiss.

The indictment of the entire structure of State Employment Insurance is based upon the following five points:

(1) The scheme broke down financially in face of the first crisis which it was forced to meet. As a result of favorable employment conditions there was a surplus of over £21,000,000 by 1920. The entire surplus was wiped out in one year, and by the end of 1922 there was a deficit of over £15,000,000. The scheme has, therefore, proved to be unsound financially. (Page 29.)

(2) To meet the post war conditions so many extensions of the benefit scale were made and so many of the qualifications removed that what remained was an insurance scheme in name only and not in spirit.

In addition to the original act of 1911 nine acts were passed in the period of 1914 to 1923, six of these between 1920 and 1923; practically all of them aimed to make the act more liberal. Taking for the sake of brevity the weekly benefits for men only, they amounted under the original act to 7*s*. per week, were increased in 1919 to 11, in 1920 to 15, and in 1921 to 20, though later reduced to

15*s*. again. The number of weeks' benefits allowed in one year in the original act was 15. It was increased to 24 in March, 1921, to 33 in July, 1921, and to 46 in March, 1923. The original requirement that there should be five weeks of contributions for one week of benefit was abolished in March, 1921. Another requirement of 26 weeks of employment in the preceding five years was repeatedly modified until in April, 1922, a very weak and flexible requirement of "a reasonable length of time of employment since December 31st, 1919" was substituted for it, etc., etc. The effort to adjust the total contribution from the three parties involved (the employer, employee and the state) to meet this increased liberality of benefits were not sufficiently stringent. The total contribution rose from 6.67*d*. in 1911 to 10*d*. in 1920, 1/1.75*d*. in March, 1921, 1/6.75*d*. in July, 1921, and 2/1.75*d*. in April, 1922, but did not meet the increased cost as shown by the repeated developments of large deficits. (Pages 68 and 69.)

(3) In face of all these extensions the Unemployment Insurance System failed to meet the problem of unemployment relief. Outdoor poor relief became necessary and grew to very large dimensions. Simultaneous relief by both the insurance scheme and doles system was common. The number of persons receiving outdoor relief increased 600 per cent in 18 months after the unemployment insurance act of 1920 went into effect, approaching nearly 1,200,000 persons in June, 1922. (Pages 81–83.)

(4) While the so-called insurance scheme was largely a disguised system of government doles, it called for a general burden of heavy administrative expenses because of its cumbersome details, equalling £7,300,000 in 1922, which could largely have been prevented through a system of doles.

(5) The combination of the insurance system with employment exchanges and the gigantic burden of clerical work which the act and its many amendments have put upon these exchanges, has almost destroyed the usefulness of the exchanges as agencies for placement of unemployed and thus paralyzed a system which was particularly needed during a period of industrial depression.

In support of these five contentions Mr. Morley marshals an almost overwhelming body of facts. Many of these contentions must be admitted by an unprejudiced student. The question remains as to the proper interpretation and valuation of these facts and their significance for an unprejudiced appraisal of the system as a whole, the circumstances which it had to face, and of the wisdom of the measures which had been taken to save it.

What happened to the system of Unemployment Insurance in Great Britain may be briefly summarized as follows:

It was created in 1912 for a limited number of trades covering about 2½ million employees. It had the good fortune of encountering a series of "fat years" with a very low unemployment ratio, as a result of which it accumulated a very substantial surplus of over £20,000,000. In face of such seemingly favorable conditions the act was extended in 1916 to cover some 4,000,000 employees; and again in 1920 a further extension was made to cover 12,000,000 employees. Then, something happened for which I imagine Mr. Morley would not hold either the authors or the executives of the unemployment insurance scheme responsible. There came an economic crisis such as England has per-

haps never seen before. The unemployment ratio, which for 37 years averaged 5 per cent and never reached as high as 8 per cent, in 1921 and 1922 amounted to over 15 per cent. That of itself would have put any insurance scheme under the severest tests. Added to it were several very important factors. Reserves had been accumulating for 2½ million persons insured. When the crisis developed, the number of insured were 12 million. The increased cost of living made the earlier benefit scale, not too liberal in the beginning, altogether insufficient. The general restlessness of millions of returned soldiers required instantaneous action.

It is in the light of these conditions that the somewhat hectic policy of parliament in regard to insurance legislation must be judged. A policy of *laissez faire* in regard to the appalling amount of human destitution resulting from unemployment was unthinkable. The choice lay between the adjustment of unemployment insurance and direct outdoor relief. The government has selected the first method even though it was not very consistent about it, and to some extent outdoor relief had to be depended upon to supplement what the insurance scheme could not handle.

It is conceivable that an equal amount could have been distributed directly without any change of the insurance scheme, but that would have resulted in a complete discredit of Unemployment Insurance. It is true that the strain, both administrative and financial, was great, but the final result was that the insurance scheme has survived, and the practicability of Unemployment Insurance as a principle has been proven; that is a result of very great social importance which must be admitted in face of all possible faults of actuarial and administrative detail. As normal conditions of employment are gradually being reëstablished, the time has come for a careful and painstaking reconstruction of the entire body of insurance legislation.

So much for the general principle of Unemployment Insurance as viewed from a point of view of social policy. The shortcomings of the British system are of particular importance to the actuary because in the final analysis, the reviewer believes it was the actuary rather than the statesman who was responsible for the comparative failure or at least the critical condition of State Unemployment Insurance in Great Britain.

Undoubtedly, expert actuarial advice was utilized when the scheme was originally repaired, and there is no intention here to question the accuracy of the various mathematical processes by which the final results as expressed in contributions and benefits were arrived at. But the accuracy of actuarial calculations is bound to be very misleading unless based upon sound statistical considerations. Surely no statistician of labor conditions could have claimed 15 years ago that enough material was available for an accurate estimate of the cost of Unemployment Insurance. The embodiment of fixed rates in the unemployment bill was as dangerous as were similar rates in the Health Insurance law, as would be rates fixed by law in an American compensation insurance act. A uniform rate in face of very diverse unemployment ratios was as unscientific as would be a uniform compensation insurance rate for structural iron work and the clothing industry. It is the responsibility of the actuaries that they have

given the legislator a false sense of security in approving such rates as part of the law.

Perhaps less responsibility attaches to the fact that those rates were expressed in monetary units rather than in percentages of wages, as is the practice in American Compensation Insurance, or as was done in both Unemployment Insurance plans in Chicago and New York. While the fixity of a price level should not have been assumed for too long a period, nevertheless, no one in 1911 could be held responsible for failing to foresee the possibility of such wild fluctuations in rates of exchange and cost of living as have taken the place even in Great Britain during and after the war.

With the author's very emphatic and frequently repeated assertion that with the extension of benefits and the removal of various restrictions and limitations the insurance scheme lost its insurance character, the reviewer is quite unable to agree. Most American compensation laws have gone through a series of revisions of extensions and increase of benefits. The requirement of certain length of insurance, of a certain number of weeks of payment of contributions prior to grant of benefit, or the limitation of weeks per annum during which unemployment benefit can be paid are not of the essence of an insurance plan. These or any other limitations or restrictions may be exacted or waived at will according to the degree of financial solvency. The main reason for the financial collapse which threatened the unemployment scheme but was avoided by a government loan of £30,000,000 was, after all, not the removal of all these restrictions and limitations, but the essential fact that during the "fat years" the insured numbered only 2½ million, while during the "lean years" the number was nearly 12 million or almost five times as many. Mr. Morley, himself, after a rather careful computation admits that had the number of insured from the beginning been 12 million, "there would have been a net balance of some £67,000,000 at the end of the eighth insurance year and such a balance would have been far more than ample to have maintained the fund in solvency up to the end of the tenth insurance year, even with the increased benefits and the lax restrictions which were allowed during the depression period." Under such conditions it would have been quite impossible to speak of the breakdown of the insurance scheme or of its degeneration into a system of doles.

Even as it is, the financial situation has not become so critical as one might imagine from reading the text. The total advance made from the treasury to the insurance fund was below £30,000,000. While the amount is not an insignificant one, yet in view of the size of the operations which in 1922 exceeded £60,000,000 the deficit does not appear alarming and can be wiped out in one or two years under favorable employment conditions and with a proper adjustment of rates.

Nor would an American reader be inclined to attach too much importance to the point of view that the insurance scheme must be condemned because out of door relief for a time remained necessary. With all the efficiency of American compensation laws, it is a matter of common knowledge that the need for public or private relief in industrial accident cases has not disappeared, for the simple reason that the benefits granted frequently prove insufficient. This may

properly be used for further increase of benefits, but surely does not argue against the efficiency of the compensation principle. The same reasoning would seem to hold true in cases of unemployment insurance.

Notwithstanding his very jealous defense of the insurance principle against doles, Mr. Morley appears not to be fully familiar either with insurance or actuarial principles. This is rather eloquently illustrated in Chapter 7 and Table 8 on page 95, which represents a very curious example of faulty statistical reasoning. An effort is made to prove that it would have been cheaper to give direct doles of the same amount from the treasury than to pay benefits through the insurance system, and a very complicated table is presented to prove that contention, which in the author's opinion is supposed to show that a system of direct doles would have cost only between 85 per cent and 98 per cent of what it has cost to the insurance system. To prove that the table calculates: the maximum benefit payable during one year under each act, deducts from it the maximum contributions from employer and employee for the remainder of the year, obtains the difference which it calls the potential deficit for a year, divides that potential deficit by the maximum number of weeks of benefits allowed through the year. This potential deficit being only slightly lower than the maximum benefit (by the amount of contributions) the proportion falls somewhat lower than 100 per cent. Absolutely nothing is proved except that for the individual workman who receives the maximum benefit the amount of his and his employer's direct contribution is only a small proportion of the benefit, which proportion gradually grew smaller as the maximum benefits were extended. The computation is about as useful as would be a comparison between the fire insurance premium and the amount of fire insurance collected on an individual case.

The final charge that because of the complexity of the insurance scheme employment officers were overburdened and could not properly attend to the business of placement, is perhaps of less importance from an insurance point of view than from a point of general policy. The evidence is not conclusive. It is true that in 1911 there were 607 placements per staff member and in 1923 only 96, but admittedly as the employment exchanges were directly charged with both duties of placement work and insurance work, a large proportion of the personnel should be charged entirely to the cost of administering insurance. Throughout the twelve years, 1911 to 1923, employment exchanges succeeded in filling from 66 per cent to 86 per cent of vacancies notified, which is an amazing rate of efficiency. The proportion between number of positions filled and the number of unemployed people registered is, of course, very much smaller and became very small during the years of severe unemployment 1921 to 1923, which is the experience of all employment agencies throughout the world. Moreover, Mr. Morley admits that the fault lies mainly with the employers who for various reasons dislike to utilize employment agencies and prefer more direct methods of employment, particularly during periods of wage difficulties.

The author's strictures on the present system may partly be explained by his point of view which is expressed in the sub-title to the book, "A Study in State Socialism." After all, the sub-title presents "a name" rather than a definite

economic theory. There would be a considerable difference of opinion as to whether the British system of Unemployment Insurance is an expression of state socialism or any other kind of socialism, any more or less than the alternative which he seems to favor, that of an Unemployment Insurance System through the industries themselves. Presumably, this alternative suggestion would also be established by virtue of law, and there might be a great many socialists of the more radical kind or brand who would prefer insurance by industry rather than directly by the state. The whole problem of the comparative advantages of the two methods may be studied from Mr. Morley's book in connection with Professor Joseph L. Cohen's "Insurance By Industry Examined."

With the advent of Unemployment Insurance in this country, Mr. Morley's study is of very great importance to the members of the Statistical Association in drawing a picture of the very serious consequences which may result from lack of well informed statistical and actuarial advice. There is no dearth of competent actuaries in Great Britain. Nevertheless, the insurance legislation of 1909 and 1911 demonstrated serious errors of statistical judgment; perhaps because there wasn't available at the time (and still is not available in Great Britain) an open forum for discussion of the statistical facts underlying all forms of social insurance such as the Casualty Actuarial Society offers in this country since its organization in 1914. Surely, careful studies in various industries would have prevented the almost catastrophic error of fixed rates of contribution in the law. The American experiments of 1923 and 1924 have at least avoided that error, probably due to better actuarial advice.

I. M. RUBINOW

Handbook of Mathematical Statistics, by H. L. Rietz, editor-in-chief, H. C. Carver, A. R. Crathorne, W. L. Crum, James W. Glover, E. V. Huntington, Truman L. Kelley, Warren M. Persons, and Allyn A. Young. New York: Houghton Mifflin Company. 1924. 221 pp.

It may be questioned whether the statistical methods are sufficiently developed to be effectively treated in the codified form of a reference handbook. Much of the technique is still in crude and cumbersome stages; many of the accepted measures and units are arrived at only by complicated and laborious computations; indeed, the very meaning and significance of the most important of these measures is still vague and but dimly comprehended. Slightly different processes and slightly different results, which purport to give these measures, are still matters of controversy. And most important of all, certain parts of the field are so little explored that what promise to be among the most useful concepts, theories and formulae, are still coming to light.

But it cannot be questioned that in the *Handbook of Statistical Mathematics* a worth-while attempt has been made toward the presentation in summarized and codified form, of formulae and theories which have been found useful in statistical work. A bold enterprise has been carried out with commendable skill and a satisfying measure of success. Within two hundred pages of large type, may be roughly counted over two hundred and fifty formulae, presented

under appropriate topical headings and apparently fully indexed. Concise explanations of the "laws" and ample development of the various theories and concepts involving these formulae introduce them. Sometimes where the procedure for the calculation of statistical measures is complicated, or the use of short cuts is useful, we find set forth illustrative sample procedures which can readily be used as guides or models.

The book stands more or less alone among general statistical texts in not being suitable for the beginner, and this is as it should be for a handbook or reference manual. But it is sufficiently elementary so that a statistician, no matter how long ago he has forgotten a process or an equation, may turn to it and refresh his memory quickly. It is sufficiently explanatory so that he can readily grasp branches of the science which he has not previously explored. It is sufficiently technical so that if we were allowed to keep but one book on statistics we would probably have to keep this. These things are, we believe, proper criteria of a handbook.

As is natural for the first attempt at a codex of a young science, the book is bound to disappoint the technician who seeks in it the last word in his own particular field of statistics. The actuarial and interpolation formulae are necessarily but a brief résumé, by Glover, of the voluminous processes used in constructing life-tables, though they occupy quite the longest chapter in the book. The sections on frequency distributions and curve-fitting are exceptionally clear-cut, but do not develop more than a skeletal description of the various types. The section on simple correlation is the best we have read, but Kelley's chapter on multiple correlation, while it greatly clarifies the presentation of the equations necessary for the determinantal solution, omits the methods of approximation, one of which is that writer's very great contribution to the science. The short chapter, by Persons, on historical series is so good a restatement of his method used at Harvard, that we wish he could have had space to describe other methods of isolating seasonals and had not entirely omitted the very important use of related data as trends, which is the most important recent addition to his method. The section on index numbers could not, of course, give all of Irving Fisher's researches.

The fact that it is the work of nine co-authors, each individually responsible for certain sections, has had other results beside giving the book wide ground. It has saved it from the prevailing sin of most writers of statistical texts, in that the illustrative material is not exclusively drawn from a particular field of statistics. An unfortunate consequence is that the method of treatment varies in different sections, being short and choppy and excellently fitted for quick reference in certain sections, and in other places more of a connected narrative designed for the step by step development of the subject; these differences being not so much due to the exigencies of the subject matter as to the idiosyncracies of the particular section-writer. With the same lack of purpose the exposition is wholly theoretical sometimes and at other times employs the case-method only. Lastly there are, as may be expected, some few repetitions, where different authors have touched upon the same ground. In these faults, the wonder is that they have been present to so slight an extent, considering that

coöperation between distant co-authors is difficult, and that editorial revision cannot greatly alter the structure and style of finished contributions.

Chapter I, by Huntington, is an excellent summary of definitions and useful mathematical laws, the brief discussion of each being in some cases better than the subsequent repetitions by other authors in their fuller discussions. Chapter II is an elementary outline, by Rietz, of statistical methods up to, and introducing, the measures of scatter in a frequency series. It includes a good method for the calculation of the standard deviation, but is at times prone to adopt as settled a terminology which is not yet determined, and derives the frequency curve in a popular, but nevertheless in our view, undesirable way. We prefer Chaddock's method.

The next four chapters form a very good, clear, and consistent treatment. Chapter III, by Glover, covers interpolation, summation and graduation formulae very clearly and offers to statisticians in all fields what everyone needs, namely a thorough account of possible ways of smoothing raw data. In the next chapter, by Huntington, the method of least squares and the method of movements are fully explained as they occur in curve-fitting, and the types of mathematical curves which can be put in straight line form are tabulated from the works of Lipka, Running, and Karsten. Two brief chapters, by Rietz, on random samplings and compound distributions, contain good reference tables of the probable errors of various statistical constants and good discussions of tests of the goodness of fit and criteria of the constants. A last chapter on the frequency series, by Carver, carrying the subject briefly into the much more difficult field of generalized forms of the uni-modal distribution, does not succeed in so clear a presentation, but contains essential formulae.

Three chapters on correlation follow. The first on simple correlation, by Rietz and Crathorne, is an excellent résumé of the subject and is perhaps the most finished piece of work in the book. In very short space and in wholly correct order are brought together almost all the important constants serving to measure association and the numerous forms of the equations by which they may be found. Scant treatment of bi-serial r and the omission of bi-serial η seem the only flaws in this chapter. The chapter following, by Kelley, presents the determinantal solution of multiple correlation in the Kelley terms and symbols, in most concise, clear-cut form, but does not identify his symbols with those of other writers. In it he has omitted his method of approximations and does not mention other approximation methods, but he has added a short discussion of multiple ratios. The last chapter, by Persons, discusses the application of correlation methods to historical series after eliminating fitted secular trends and the link-relative type of seasonal.

Three miscellaneous chapters conclude the book; one on periodigram analysis, by Crum, giving the procedure for the determination of the constants of periodic functions; another on index numbers, by Young, and lastly a bibliography, by Crum. The chapter on index numbers is an excellent outline of the subject and contains very clear presentations of the better formulae and critical discussion of their merits. At the end of the book is also given a table of the second, third and fourth derivatives of the normal curve, reprinted from Glover. The index

to the book seems adequate and thorough. A number of minor typographical errors have slipped into the first edition and readers should obtain from the publishers an errata slip intended to correct them.

It will be seen that as usual the frequency series gets the lion's share of the book, and the historical series is neglected. This is the result of the importance of the averages and measures of dispersion and correlation which are developed under the head of frequency series. It may also be considered that the technique of historical series has not been sufficiently developed and standardized to merit a place in a handbook. But it leaves a book in which, what is to us the more important half of the subject matter of modern statistics is inordinately slighted. In future editions there will have to be additional chapters on the historical series.

An Introduction to the Mathematical Analysis of Statistics, by C. H. Forsyth. New York: John Wiley & Sons. 1924. 241 pp.

Statistical textbooks have in the past fallen into two fairly sharp groups, neither of which exactly is designed for the needs of the classroom. In the first group belong those which contain new and original matter presented for the first time by the author. These books are often excellently suited to classes in all parts except the original ones, but in these original parts the author is apt to shape the development along the lines of his own research, and show how he came upon the original methods or theories he is presenting, rather than to shape the development along the lines most useful for practical classroom work. In the second group belong less technical books on statistics which have been written in an elementary and somewhat diluted way for the general reader, rather than for the student. In other words, statistical treatises have either fallen beyond the classroom mark, or fallen short of it.

In Forsyth's book we have the first of a new class of statistical texts, namely a book explicitly designed and really suited for courses in statistics in schools and colleges. For the first time the statistical methods have been presented with that well-known, cut-and-dried, pedagogical style in which the algebras and arithmetics which are used in classrooms, are written. The text is divided into short sections suitable for the hourly session, and covers the field in a straightforward, thorough, workmanlike way. Indeed, it succeeds in being surprisingly interesting for a class text, but this is entirely incidental to the main purpose of setting forth the subject in the clearest, shortest way. There is no better textbook in our literature for the elementary courses in statistics. There are no frills, no original matter, no new theories or methods and, as the author says, "no controversial matter." On the other hand, there is no dilution for the layman, no writing down to make continuous reading easy.

Written by a former student of Professors Rietz and Glover, the book follows the same outline of the subject as does the *Handbook*. Thus the first quarter of the book is given to significant figures, finite differences, interpolation and summation formulae, and contains excellent and rather complete treatment. This part of the book presupposes a knowledge of the principles, at least, of the calculus. A good chapter on probability follows. The remaining two-thirds

of the book cover the customary ground of the statistical methods, namely averages, moments, the normal curve, dispersion, skew, and correlation. A short statement of multiple correlation is appended, with the recommendation that it be omitted from the course.

As in the *Handbook*, the historical series receives little attention. There is a section on index numbers in the chapter on averages. Weighting is discussed and Professor Fisher's "ideal formula" is given. In the chapter on moments, there are sections on seasonals and secular trends, setting forth concisely the Persons method, described in the Harvard publications. It seems that in the Rietz-Forsyth scheme of statistics there are no other worth-while ways of isolating seasonals and trends and that Professor Persons has closed the subject. In this respect the book has failed to avoid controversial ground, and has failed to indicate that controversy exists. A serious mistake is made in equipping students for practical statistical work with no more knowledge of the historical series.

The book is excellently suited for the class in statistics meeting three or more hours a week for a full year. It is necessarily elementary, but good references are invariably given for the student who wishes to specialize on any topic. It seems fair to say that a knowledge of the why and how of practical statistics is assumed, as well as a working knowledge of calculus. Thus there is no discussion of sources of statistics, methods of research and collection of data, there is no explanation of the principles of correct tabulation, and no explanation of charts and graphs. The book treats of the mathematical technique of statistical work, and it is probable that drill and groundwork in the foregoing omitted phases of statistical work can be given informally in the classroom when needed.

The style is clear and illuminative. Especial praise is due to the author for the copious "exams" and questions at the end of each section; they seem to be carefully selected from years of experience in classroom work and they would form a valuable fund of assignment problems for any teacher of the subject, whether or not the book is being used as a text. So far as we know, this is the first book on statistical methods to provide adequate exercises for class work.

Elements of Statistics, by Frederick C. Kent. New York: McGraw-Hill Book Co. 1924. 178 pp.

This small book is a very elementary text on the statistical methods—how elementary can be seen from the fact that it is expressly designed for classes meeting two hours a week for one semester only. It is a textbook proper, with short disconnected sections and simple exercises scattered through it, and, in its appendix a three-place table of squares, square-roots, cubes, cube-roots, and a three-place table of the ordinates and areas of the normal curve. Students raised on such pabulum could not be statisticians, but would no doubt have a speaking acquaintance with some of the simplest terms. For high school and vocational courses, turning out statistical clerks, this book is excellently designed. For general cultural purposes in a college course, we would prefer the more connected works of King and Secrist.

The limitations of this little book are clear. It contains 134 pages of text in

very large type. Its section on index numbers does not scratch deep enough to reach Fisher's formula. It has no discussion of frequency series types other than the normal curve, no discussion of correlation beyond total correlation coefficients and ratios. There is no mention of seasonals or trend in historical curves, though the moving average is described. Interpolation is limited to graphic interpolation. In general, there is a somewhat dangerous tendency to accept the outstanding measures of dispersion and the like in too broad and unqualified a sense; thus the probable error is identified with the semi-interquartile range and is usually called the probable deviation. The references for students who wish to delve deeper seem to us inadequate and unsatisfactory. And the style falls short of the style of that ideal of "very simplest introductions," Thompson's *Calculus Made Easy*. This is not "statistics made easy"—it remains serious work for the pupils.

Understanding the very elementary basis of the book, one recognizes many excellent features. The first third of the book is given wisely to the introductory topics of computing machines, a hazy sketch of some of the sources of current statistics, a short talk on the principles of tabulation and two chapters on the graphic methods, the first of which covers historical curves and includes the logarithmic curve proper, the second the frequency curve. These things lead easily and naturally into the discussion of frequency series and the usual ground of statistical methods. On the whole, however, there seems little to us to justify the publication of this book. We hope that time will develop Mr. Kent's flair for textbook writing and enable him to give us a more complete, more reliable, and more thoughtful volume for the very short course in elements of statistics.

KARL G. KARSTEN

Mathematical Theory of Life Insurance, by C. H. Forsyth, Ph.D. New York: John Wiley & Sons. 1924. $1.25 net.

This book was designed primarily as a text for use in a short course, supplementary to work in finance and economics, in what the author calls "this wonderful system of long time finance." The material is highly condensed, but in such a manner as readily to permit and even suggest classroom expansion. The use of the book in the classroom seems likely to attract students to more advanced studies of the subject. Readers will probably find the chapter on valuations somewhat unsatisfactory, but nevertheless it can be recommended not only to students in finance but to professional or business men who have not entirely forgotten their high-school algebra and who find very tiresome the long-winded arithmetical explanations of the usual elementary book on this subject. The author's treatment is in excellent logical order and for the most part very clear; he has quite successfully guarded against the admission of material not essential to the basic theory of life insurance. A good example of the author's control in minimizing non-essentials is found on page 31, where he proposes for discussion as an exercise a few searching questions about assessment insurance and lets it go at that.

Dr. Forsyth's book opens with an introductory chapter on the elementary

notions of probability in which he makes a clear distinction between *a priori* or mechanical probabilities and *a posteriori*, empirical or experience probabilities. He then passes to a chapter on the mortality table as an example of the latter type of probability and shows how, by the use of one or more bases of experience probabilities, life insurance companies can forecast death rates with sufficient accuracy to discount their obligations on a safe basis.

Chapters II and III are devoted to the basic annuity and insurance formulas. By the use of a symbol nIx to represent the single premium for an insurance during the nth year the author seeks to bring out more clearly for the student the correspondences between the annuity and insurance benefits. Unfortunately the symbol he uses is one preëmpted to represent accumulated increasing insurances. On page 21 the author should have used or at least referred in the footnote to the established use of the open form of N to distinguish between the initial and terminal forms of that symbol.

The fourth chapter on policy valuations, which the author evidently thinks is his best, will probably be the least satisfactory to actuaries. He appears to attach special importance to the symbol nEx. One result of its use is that formula 34 cannot be conveniently written in commonly tabulated factors. In discussing preliminary term valuation and its modifications, Dr. Forsyth says that full preliminary term valuation is objectionable because it "would encourage extravagance on the part of the company." Another and less debatable reason is that it would result in discrimination between policyholders of the same age. The New Jersey standard described was changed in 1920 to modified preliminary term, 20-payment life basis.

Chapter V deals with mathematical expressions for the "law of mortality"; specifically with the formulas of De Moivre, Gompertz and Makeham; and logically enough includes a discussion of joint life contingencies. It would seem as if the phrases "formulas for the laws of mortality" and "Makeham's formula or law" might well have been briefly analyzed to avoid misleading the student into an erroneous idea of the meaning of the word "law" in this connection.

The author of this book is a mathematician writing about the business of life insurance and, as might be expected, he has not avoided a few actuarial infelicities. To refer to the tendency of accidental errors to compensate each other and the merging of group fluctuations in different kinds of risks into a satisfactory general result, as "the principle of mutuality of insurance," seems either obscure as to meaning or open to argument. Fortunately for life offices the companies are not, as stated on page 35, required to file with the several state insurance departments a statement of the "reserves on the policies held in the particular state." The statement on page 36 that the symbol $Ax+n$ "is simply the net cost at his attained age" is inaccurate.

Notwithstanding the defects mentioned and some others, the book is well conceived and executed and the reviewer hopes it will meet with sufficient demand to require a new and revised edition in the preparation of which the author will submit his text to some company actuary.

PERCY H. EVANS

Milwaukee, Wis.

Elements of Business Statistics, by Robert Riegel. New York: D. Appleton and Company. 1924. xx, 549 pp.

If a grammarian were to write a textbook on grammar, illustrating it with selections from the writings of Emerson, Santayana, and James Harvey Robinson, and were then to call it "Elements of Philosophy," he would be giving his book a title not much more misleading than the title of Professor Riegel's present volume. The fact that Mr. Riegel's illustrations are almost entirely from the field of business certainly does not entitle him to call the book by a name which to most people ought to connote a book dealing, among other things, with the elementary concepts of the philosophy of statistics in business. There is mighty little philosophy in the book.

Since the book pretends to be only an elementary treatment of the subject of statistics, it may be excused for not breaking any new ground or, to change the metaphor, for being merely a warming-over of the creative work of others. There is no excuse, however, in a book on statistics, even in an elementary book on statistics, for a lot of elementary errors. These errors are statistical, graphic, grammatical and typographical. The following paragraphs will mention some of them:

Doubtless the prize exhibit of the book occurs on pages 84 and 85, where Figure 37 is a semi-logarithmic chart with the scale on the left side reading up from 61 to 90, and the scale on the right side reading up from 11 to 40. Turnover of deposits in New York City is plotted against the left-hand scale, and the volume of transactions on the New York Stock Exchange against the right-hand scale, for the months of 1919. Then the text says: "Figure 37, however, shows a ratio comparison of these two series, and from it the *striking similarity* in the direction and *relative* extent of their fluctuations is immediately recognized." (Italics are the reviewer's.)

Chapter XII is on dispersion. A paragraph on page 278 deals with the range method, illustrated by pig-iron production in this country and unfilled orders of the U. S. Steel Corporation. The second sentence is as follows: "In the first place, since the average portunity for variation." This is merely wretched proof reading, but probably neither typothetae nor proof readers can be blamed for the last four sentences of the paragraph, which read as follows:

> In the unfilled orders and pig iron, the average for the former is only 6.8 million, while the average for the latter is 35.5 million; an equal variation could hardly be expected. Nor is this the only factor to be taken into consideration, for the pig-iron figures are for annual production, while the unfilled orders are on a monthly basis. If the latter had been expressed for years instead of months the average annual production would be 81 million, while the range would be about 120 million tons. Evidently the difference in the time interval used makes considerable difference in the comparison.

On page 202, Table 35 shows, among other things, that in 1920 there were 235,000,000 (*sic*) miles of Class I railroads in the United States. This might be blamed on a careless clerk but, referring to the table, the author himself says in the text that "Column 3 shows that the mileage has increased 3,000,000 miles from 1916 to 1920. . . ."

A few pages later a well-known vice-president of the Cleveland Trust Co.

and counsellor of the American Statistical Association is referred to as L. M. Ayres, and on the same page, in a reference to automobile production and registration, these two sentences occur:

> Having obtained the production in given years and the disappearances from use in successive years, we can compare the rate of increase in the former with that of the latter. If the latter rate is greater than the former the average automobile is becoming obsolete faster than formerly.

This statement on automobile "mortality," by a professor of insurance, hardly tells the whole story.

The Harvard University Committee on Economic Research must have been interested in reading the description of what Mr. Riegel calls "the so-called 'Harvard barometer.'" On page 498 the statement is made in connection with this barometer that "*business* is represented by a figure calculated from (1) the volume of pig-iron production and bank clearings outside of New York and (2) the prices of commodities as shown by two well-known index numbers of wholesale prices." Two pages later a chart of pig-iron production, corrected only for secular trend, is shown, and referring to the chart, the text says: "Thus we see from the diagram that production exceeded normal in 1905–1907 and was considerably below normal in 1908, 1911 and 1914." Professor Persons would hardly relate any production curve to normal which was not corrected for seasonal movement.

One might go on, but what's the use? The "sample" is sufficient. In the very last line in the book before the index is the square of 1,000, and this is given as 10,000,000. Let us close by calling the attention of persons interested in graphics to Appendix I, "Some Requisites for Graphic Representation." The last item in the list is: "9. Pencil. Since the pencil lines are finally to be erased it is best to use a reasonably hard pencil." Surely this *is* "Elements of Business Statistics," but curiously enough the author does not include an eraser among the requisites for graphic representation.

The chapters of the book close with very useful lists of references.

F. E. Richter

New York City

The Scientific Study of Human Society, by Franklin H. Giddings. Chapel Hill: University of North Carolina Press. 1924. 247 pp. $2.00.

The effort of sociology to establish itself as a respectable academic discipline is an interesting episode in the history of western culture. As a field of speculation and of inquiry it is as old as economics, politics or ethics. It is, in fact, a sort of matrix from which these have evolved. As old-fashioned Social Philosophy it included much that has since been appropriated by special social studies, notably ethics. These special fields of inquiry then succeeded in constituting themselves as recognized "sciences" and straightway assumed that they, among themselves, monopolized the whole field of social inquiry. For the most part they had, in fact, little claim to the dignified rank of sciences, though they were obviously important fields of human interest. They were

more or less loosely organized collections of observations, systematic descriptions, rationalizations and wishful constructions of human hopes and aspirations. To some extent they became logical systems, far removed from the realities of dynamic social life and taking to themselves an increasing air of superiority with their increasing detachment. To some extent they were systems of metaphysics with an atmosphere of esoteric profundity quite out of proportion to their real worth as possessing insight into social reality.

In the last quarter century, however, all these social sciences, and numerous new ones which have emerged with the increasing specialization of research, have taken on new airs of positiveness; they have become more and more inductive and more and more quantitative. This has been notably true of economics, under the leadership of such men as Alfred Marshall, Arthur L. Bowley, Henry L. Moore and Wesley C. Mitchell. Politics and ethics, while becoming distinctly more realistic, have made much less progress toward quantitative inductions. At the same time all three have reflected, increasingly, an awareness of the interrelations and interdependencies of one on the other, of economics on politics and ethics, of politics on economics and ethics and of ethics on economics and politics.

The development of sociology as the basic social science contributed powerfully to this latter tendency. In fact, a good case could be made out for the claim that it has been the writings of the sociologists that were largely responsible for giving new breadth and depth to the special social sciences. It was the sociologists also who developed and made most use of the Le Play monographic or survey method of inductive and quantitative social investigation. It was Karl Pearson also who claimed to be founding a new sociology when he devised and applied the technique of advanced mathematical statistics to the problem of the relative importance of heredity and environment in numerous social problems. Nevertheless, sociology has been slow to apply in fruitful ways this technique, a slowness to be explained in part by the failure to formulate significant problems, and in part by the lack of trained investigators with time and resources at their disposal.

Meanwhile there has been an enormous amount of social surveying and social investigating. Grown wealthy by a pitiless plundering of both man and nature, individuals and classes have sought psychic compensation for violation of the professed ideals of Christianity and democracy by instituting a highly variegated "uplift" whereby employment has been furnished for a small army of officious and well-meaning persons whose activities have been justified in their own minds 90 per cent by their own good intentions and 10 per cent by their ignorance of what they were actually doing. This should not be interpreted as a condemnation of all "uplift" activities, but as an effort to state vividly yet truthfully the fact that we are as a society full of hopeful "movements," but nearly empty of scientific means of measuring them and their social effects.

Since the publication of his "Inductive Sociology" in 1901 Professor Giddings has been the chief exponent of quantitative methods in sociological study. He has been also a frequent contributor to such methods and an outstanding opponent of blind leading of the blind in the rocky pathways of social "improve-

ment." As he says in the volume at hand with reference to the work of churches and settlements: "Their reports, with commendable exceptions, are a flotsam of unverified assertions, uncritical impressions, and optimistic forecasts, made, not to establish fact, but to wheedle money for more loose work of the same kind" (p. 57). Similar criticisms might be leveled at a great variety of ostensibly "good" or "beneficial" activities. Professor Giddings then proceeds not only to point the moral of the need of more exact determination of just what is going on but to advance a variety of suggestions for the classification and measurement of social phenomena.

It is a book that one will not read for pleasure. It must be studied, analyzed and pondered over; but it will repay the effort. Its style is extremely brief and compact; so much so that in many places the exposition would be greatly improved by expansion and illustration, especially the latter. There are numerous logical distinctions which are of interest and significance as studies in the logic of scientific method. The concluding chapters furnish an outline for a study of the whole technique of social surveys and statistics. And as a sort of link in the whole discussion there is at the center a chapter on "Societal Telesis" in which the question is raised whether, after all is said and done, society can by conscious volition do anything to control its own destiny.

All naïve persons, including the vast majority of social uplifters, reformers, and utopian reconstructors of society in whole or in part, would hardly think the question worth raising. For the sociologist, it is the profoundest question in his philosophy. Professor Giddings finds that social control can only advantageously carry out the processes of natural social evolution. "The telic process in human society does not, in any reasonable meaning of the word, supersede the genetic process" (p. 170). The methods of both processes are the same; so also are their causes and effects. And yet, in spite of this conclusion Professor Giddings implies that certain ends, which are evidently the ends of social evolution which he cherishes, can be realized by telic action. Even the statistically-minded sociologist finds it nearly impossible to think about society as a natural process not suffused with his own subjective idealizations. All in all Professor Giddings, as would be expected, has written a unique book. It should be studied by all professed sociologists for its social analysis, its classifications and its philosophy.

F. H. Hankins

Smith College

Graphic Statistics in Management, by William Henry Smith. New York City: McGraw-Hill Book Company, Inc. 1924

In the preface the author states: "In this book an effort has been made to treat the principles of statistical and graphical practice cohesively to show their interdependence and relation in order that the student or business man may find an exposition of the subject sufficiently complete to enable him to understand what others have done or may do and to establish within his own business a statistical and graphic practice of his own."

Through simplicity of style the author has succeeded in writing a book which will be helpful and instructive to those interested in the subject of graphic statistics. Business men who are easily antagonized by lengthy mathematical statements will appreciate the short and to the point explanations of Mr. Smith.

In Chapter I we are introduced to the simple line graph—here the reader makes his first contact with the meaning of "graph." As we go along we are not confused by technical terms. The section "statistical data" on page 6 discusses briefly the purposes for which data are collected, the statistical unit, the method and sources of business statistics, internal and external statistics, personal investigations and the questionnaire method, etc., to be followed by Chapter III, "Statistical Presentation," which explains the rules of tabulation and classes of tabulation. Types of averages are given and examples for their application.

Part II is devoted to the construction of charts. The author does not favor the pie chart on the whole, and graphic statisticians will agree when he says: "Generally, however, comparison involving two or more circles should not be illustrated by this type of graph. Better and clearer methods of presentation are available."

Chapter V describes the main types of bar charts, a good set of rules for their design is given at the end.

In the next chapter we find more details on the curve or line chart. Enough material is covered to familiarize the reader with this type of graph and a few selected charts are dissected and criticised for the purpose of showing correct methods of graphic presentation.

In mentioning presentation of component parts by means of curves, Mr. Smith gives a good rule, namely: "It is good practice to place the largest item at the bottom, the next largest above, and so on." The point is brought out well in two illustrations.

In Figure 36 the legends "follow" the trend which gives any chart a clumsy appearance and interferes with correct visualization. Legends should be written horizontally whenever possible. In Figure 37 curve legends are placed correctly, namely, parallel to the X-axis, this arrangement appeals to the eye and avoids the confusion of the legends in Figure 36.

In Figure 38, in which a cumulative or mass chart is given, the chart does not show the vertical scale unit. The statement that a "cumulative chart always runs diagonally" may be contested on the grounds that if we consider a chart showing: "Monthly cumulative net orders received," as it is frequently used in business, the item "cancellations" would certainly prevent the curve from running constantly diagonally and still the cumulative principle would not be violated. On page 70, Figure 49, in the section which discusses the logarithmic chart it seems that the graph is somewhat confusing regarding the vertical logarithmic scale. Production and Exports of Anthracite Coal are shown. The cycle provided for exports has a range 2,000,000 to 4,000,000 tons, although exports reach almost the 5,000,000 line not shown on the chart. The second line counted from the 4,000,000 line represents 60,000,000 tons (the unit is not given in Figure 49). This is a misleading method. It would have been better to assign for exports a cycle running from 1 to 10 millions of tons for the left-hand vertical

scale and to provide a cycle 10 to 100 millions of tons for the right-hand vertical scale, assuming, of course, it is desired to get along with one logarithmic cycle. The chart shown in Figure 49 gives the impression that one cycle is telescoped into another one.

On page 73, Figure 52, no unit is given for the vertical scale.

On page 79, Figure 58, and Figure 58a, the author gives a diagram and a table which are not only interesting from the standpoint of logarithmic plotting, but also on account of the relationship, production, shipments, sales and stock on hand—which are some of the most important movements for the practical business man. Another phase is that of unfilled orders, new orders received, cancellations and shipments which the author does not mention here.

Perhaps it would have been well to familiarize the student with and to recall to the business man the two simple equations which govern these two important stages, namely:

(1) Stock on hand at the first of the month (or week as in table 58a), *plus* production during the month, *minus* shipments during the month, *equals* stock on hand at the last of the month.

(2) Total net unfilled orders at the first of the month, *plus* new orders received during the month, *minus* [cancellations during the month *plus* shipments during the month], *equal* total net unfilled orders at the last of the month. Furthermore, sales and shipments have frequently a distinct lag, therefore sales should not be considered to represent shipments as it is done in the diagram. In case there exists a considerable lag between the two, it gives the executive the valuable information how long it takes to get shipments out. To avoid confusion in the interpretation of the logarithmic control charts the data should also be plotted on an arithmetic basis. In charting production, etc., thus, it seems well to start the cumulative curve of production at the point of stocks on hand. In the case of table 58a, cumulative production for January 7th would start at point 200 (weekly production), *plus* 1000 units stock on hand *equals* 1200 units, therefore the curve should start at point 1200, and not at point 200.

Now, in plotting the data in this manner, a distinct point is gained. The arithmetic-graphic difference between the cumulative production line and the line representing cumulative shipments, equals the amount of stock on hand at the last of the month, or in this case at the last of the week. Suppose now that the production rate is cut down, or production stopped, as it is often the case in times of business depression, the projection of the shipment line to its intersection with the production line will show the executive how long production can be decreased or stopped until the stock reaches the minimum zone, or until it is entirely exhausted. The weekly stock curve represents, therefore, the graphic differences between the cumulative production curve and the cumulative shipping curve if plotted arithmetically.

As additional information the minimum stock line can be graphically entered in the proposed supplementary arithmetic chart by plotting the constant minimum stock magnitude (500 units), using the cumulative shipment line as base and connecting the new points which represent the minimum stock curve.

Any time the production curve runs below the "minimum stock curve" the fact is graphically visualized without having to refer to the table.

After a short chapter on the cumulative chart devised by A. R. Burnet, the author continues to describe map charts. On page 99, Figure 71, we find a map showing the distribution of traffic on Maryland's state highway system. This is a good example to show that type of data, but the insert charts A, B, C, D, E and F are diagrams intended to show the seasonal variations of traffic. Polar coördinates are used here and the central area, which is formed by the boundary of what is supposed to be, trend, is shaded.

The use of polar coördinates is very limited in the statistical field, and I am sure that these charts have escaped the author's attention, because neither trend nor area are of any significance in this type of diagram. In the case, to draw on the engineering field, where the contour represents, for example, a rotating cam to operate the lift of a gas engine valve, the contour is of course highly significant.

In Chapters XIII and XIV, "Statistics of Business Activity," the author reviews in succession such items as Idle Cars, Bank Clearings, Failures, Building Permits, etc., thus familiarizing the reader with their importance and significance as business indicators.

A chapter on "Statistics of Financial Activity" is followed by one on statistics of basic commodities and by a chapter "Statistics of the Industry," which concludes Part III.

Finally in Part IV the author describes the graphic application of statistics. Space does not permit a detailed discussion of this section. However, attention must be called to Figure 149 on page 227. The note at the bottom of the chart does not explain why the scale of the second chart is twice as large as that of the first and third charts.

On page 228, Figure 150, the legends are poorly organized.

On page 294, Figure 193, no vertical scale caption is given, the legend referring to calls says: "Average Number of Calls," but it should be : "Average Number of Calls Per Day," as stated in the text. The meaning of the time scale is also obscure. A chart can only be of value and carry the message when every detail has been correctly represented.

On page 295, Figure 194, no vertical scale caption is given, the veritcal scale starts with 2500 as a unit, no break indicates that zero has been omitted.

The object in bringing out these defects is that I feel that the name of Part IV, "Graphic Application of Statistics," entitled this section to special consideration. Only classic examples of graphic presentation should have been incorporated.

All these charts should have been subjected to the recommendations given by the Joint Committee on Standards for Graphic Presentation of the American Society of Mechanical Engineers, quoted on page 61 of the book.

Let us remember that graphic charts represent the telescope of the statistician. Properly planned, they do not distort the statistical universe but a faulty design carries all the errors of optical refraction.

R. von Huhn

Washington, D. C.

The Reparation Plan, by Harold G. Moulton, with the aid of the Council and Staff of the Institute of Economics. New York: McGraw-Hill Book Company, Inc. 1924. pp. x, 325.

This book ought to do a great deal to bring the salient features of the "Dawes Plan" within the ken of large numbers of intelligent people to whom the actual reports of the first and second (Dawes and McKenna) committees of the Reparation Commission looked too formidable and too tiresome for direct assault, and whose information hitherto has therefore been confined to the rather unsatisfactory epitomes in the newspapers. For those who seek chapter and verse the committees' reports are reprinted in full along with a useful index, but the author has devoted the first part of his book to presenting their substance in concise, well-organized and readable form, with a running fire of explanatory comment which is both just and instructive. He then proceeds to an appraisal of the revenue features of the Plan, followed by a consideration of the transfer problem and an examination of certain unsettled aspects of the reparation question. The Plan carries internal evidence of contradictory views on the part of its framers, and unfortunately the unsound view is sometimes dominant. Dr. Moulton criticizes with deserved severity the sophistical argument that by inflation the Germans have escaped the tax burdens which the Allied Nations have borne, and that Germany's capacity to pay reparations has been increased thereby. He emphasizes, too, the profound difference to any given nation between paying a debt to its own citizens and to foreigners. The latter diminishes the national dividend, the former does not. The estimate by the committee of the prospective receipts from railroad revenues is felt by the author to be much too high, and he adduces sound reasons for his opinion.

In general his appraisal of the revenue features of the Plan commands approval. But when he comes to the transfer problem, he provokes, from the reviewer at least, emphatic disagreement. It will be recalled that the Plan sharply separates responsibility for the raising of reparation revenues from that of their transfer. The duty of raising the revenues is Germany's; the task of transferring them to the use of her creditors falls on the latter. Dr. Moulton is, on the whole, disposed to think that the estimated revenues may be raised, but that it will be wellnigh, if not absolutely, impossible to transfer them. The reviewer is convinced that, provided a sensible attitude toward German exports can be induced in the creditor countries, the latter is much the easier problem of the two. Of course if all other nations exclude German goods by tariffs high enough to amount to prohibition, no reparations will ever be paid. But if they really want reparations and the funds to pay for German goods are placed to their credit in Germany by the Germans, they can get their reparations without serious difficulty. Dr. Moulton seems to suppose that they must passively wait until in some inconceivable way Germany develops an excess of exports sufficient to cover the reparation payments. If they wait for this to happen, they will wait a very long time indeed. They must *induce* those exports, and the way to induce them is to draw on their credits in Germany by selling exchange. If prices are too high in Germany to develop the necessary exports of goods some of the gold reserve must be taken instead, until by the restriction of credits, prices in Germany fall to the

requisite level. This will be the main source of difficulty and must be handled with great circumspection in order to prevent a run on German banks, the abandonment of the gold basis of the currency, and another precipitous decline in the exchange value of the mark. But this disaster must be risked if reparations are to be obtained at all. The Transfer Committee must release to the Allied creditors so much exchange as will keep the mark continuously at the specie export point, with occasional excursions below it, until the price level in Germany has been adjusted to an excess of exports equivalent to the desired reparation payments. If any tendency to a run on German banks appears, it would be sound policy for the Allied creditors to make temporary loans of gold to Germany adequate to allay the disquiet. Dr. Moulton is sceptical of conventional theory in this matter on the ground that it seems to him not to offer an explanation of the events of the past few years, but for this there are reasons not inconsistent with that theory, into which it would be superfluous here to enter. In any case, the author provides no alternative hypothesis. To his argument that Germany cannot develop the requisite excess of exports because her exports are in the main fabricated from imported materials, it is enough to point out that a relatively small quantity of highly manufactured goods will be equivalent in value to the raw materials of which they are made, and that in the past a large share of the imported raw materials have gone into manufactured goods consumed in Germany. Some of these must now be exported, if reparations payments are to be made, and the home consumers must do without them.

The chapter on Unsettled Aspects of the Reparation Problem indicates the difficulties still to be met. No total of reparations has been set and it is within the province of the Reparation Commission to declare that, even if the Plan is fully carried out, the payments made do not even amount to interest on Germany's acknowledged debt to the Allies. No limit has been placed on the number of years the payments are to run, the amount already paid is in dispute and the principle of arbitration has been invoked only with regard to the payments to be made by the railways. The success of the plan hangs on a permanent spirit of mutual accommodation which so far as history is any guide, is scarcely to be expected.

FRANK D. GRAHAM

Princeton University

Statistical Methods Applied to Economics and Business, by Frederick Cecil Mills. New York: Henry Holt and Company. 1924. 604 pp.

Professor Mills' "Statistical Methods" will probably receive wide recognition as the best textbook dealing with the application of elementary and intermediate statistical technique to the problems of economics and business in America.

The student is introduced to the essentials of methodology in the field of economic and business statistics. The subject matter is well ordered and clearly presented. Each discussion has been supplemented with carefully prepared tables and charts. The illustrative material is refreshingly original.

In this brief review we cannot enter upon a discussion of details, but the au-

thor's exposition of the relation between the ogive and the frequency curve (pages 93 to 96 inclusive) may be cited as an example of the excellent pedagogical treatment adopted throughout the book.

It is good to note that the "Charlier Check" has been included in the calculation of the standard deviation (pages 156–157). In a second edition, Dr. Mills may desire to adopt other materials from Charlier's text, especially his correlation table, which is particularly designed to aid the student in organizing the details of computation.[1]

If any further adoption of contributions from foreign authors were to be considered it is possible that the elementary and intermediate sections of texts by Knapp, Lexis, Czuber, Forcher, Bachelier and Wicksell might prove valuable. At the present time one of the large statistical offices in this country is giving careful attention to the methods of Charlier and Wicksell.[2]

The chapters of Professor Mills' text dealing with time series contain a frank recognition of the fact that there are many "knotty problems" as yet unsolved. The methods now in use have been faithfully outlined and critically discussed. The bibliography on the subject of seasonal variation has been carried to the latest possible date. It is quite likely that the author will be criticized by some readers for his application of the term "normal" to an adopted trend.

The discussion of the problem of estimation (Chapter XIII), dealing with the use of the arithmetic, harmonic, and geometric measures, will be appreciated by the student of economics and business. Here, as in all the other chapters of his book, Dr. Mills has considered the application of technique to the specific types of material under study.

No attempt has been made to present an exhaustive mathematical treatise, but such derivations as are given should prove to be helpful to the student.

Certain "mechanical" considerations make it possible for one to read this textbook "backwards and forwards." For example: (1) Lengthy computations and the discussion of complex themes are followed in every instance by recapitulations. (2) There is no confusion of symbols such as is sometimes found in texts of this nature. (3) The reference lists at the close of each chapter include the works of seventy-nine writers, and have been reprinted at the end of the book. (4) The index is usable.

Finally, it should be noted that Professor Mills has interspersed his entire discussion of statistical methods with many carefully written notes of warning to beginners and others who may look upon such an elementary treatment as providing a set of easy solutions for some of the complex problems of economics and business.

MELBOURNE S. APPLEGATE

New York City

[1] Charlier, C. V. L., *Vorlesungen uber die Grundzüge der Mathematischen Statistik*, Verlag Scientia, Lund, 1920. p. 95. (Stechert, New York.)

[2] Wicksell, S. D., *Elementen av Statistikens Teori*, Svenska Försäkringsföreningens Förlag, Stockholm, 1920. (Stechert, New York.)

Statistical Method, by Harry Jerome. New York: Harper & Brothers. 1924. xxiv, 395 pp.

This book was written primarily for students of economics and sociology. However, the author has not avoided explanations of mathematical processes, but, without giving rigorous demonstrations of their validity, has presented them in such a way as to make clear their significance.

The questions of tabular and graphic presentations of statistical facts and the analysis of data are taken up before the question of collecting data. This order seems both desirable and logical, for the experience of the reviewer has been that of the author as stated in his preface, viz., that the question of where to find statistical data is of little interest to the student until he has obtained at least an elementary idea of how to analyze them.

The concept of sampling is introduced early, and the importance of the sampling process in statistical method is emphasized.

A good chart or table conveys its message so well that the reader seldom analyzes why it does so. He is quite apt not to realize that its construction involves the intelligent observance of certain rules of technique. The whole subject of classification, tabulation, and graphic presentation is quite capably treated by Professor Jerome, who explains in some detail what constitutes a good table or graph and how to prepare one. The discussion of graphic presentation is concluded with a good explanation of ratio or logarithmic charts—how to construct them and how to interpret them.

The idea of continuous and discontinuous variation is clarified by the use of the terms *strictly continuous*, *strictly discontinuous*, and *approximately continuous* series.

The subject of averages, at least mean, mode, and median, is well presented, the author giving a very thorough discussion of the advantages and disadvantages of these averages for various purposes. His special mention of the median of a historical series is to be commended. There is a good discussion of ratios and coefficients.

On page 121 occurs the unfortunate statement that "the median is the middle-sized item." That this is incorrect is illustrated if we consider a group of five coins, consisting of three dimes, a quarter-dollar, and a half-dollar. The quarter is the middle-sized item, yet it is not the median. If the statement referred to were deleted, the remainder of the paragraph in which it occurs would stand as a good definition and explanation of the median.

The author fails to emphasize the advantages of employing the class interval as a unit in computing the mean and the standard deviation from a frequency table. The class interval should certainly have been used in connection with Tables XXIX and XXXVIII, as the numerical work would have been considerably simplified.

Formula (13) (pages 155 and 385) for the average deviation should be made more general by making it apply not merely to the average deviation from the median; moreover, it should make clear that signs of deviations are ignored.

The Theory of Probability and Error is well treated, which is difficult to accomplish in a non-mathematical treatise.

Professor Jerome wisely begins his treatment of index numbers "with a descriptive study of the more common *forms* rather than with the principles determining the choice of forms and methods." In a chapter on American Index Numbers he discussed specifically certain of the most important of these numbers.

In the chapter devoted to Seasonal and Long-Time Trends he takes up the simple average method, the typical deviation from trend-cycle method, and the link relative method of determining normal seasonal variation, and gives the advantages and disadvantages of these methods. This discussion of advantages and disadvantages of methods is indeed a typical and commendable feature of the book.

In dealing with Business Cycles and Barometers the author discusses the Harvard business index, the Brookmire Service, and the "Babsonchart," although he states "that it is not a necessary implication that the three which are singled out for illustrative purposes are all necessarily superior to those not discussed."

In these chapters on time series he gives a full discussion of some of the newer developments which have taken place in statistical method. Much attention is given to time series, which is desirable in a treatise written chiefly for students of the social sciences.

The use of the term *mean sum-product* of the deviations for the product-moment or Pearsonian coefficient of correlation is unfortunate. If a new term must be coined, *mean product* would be better; we do not speak of the mean of a *sum* of quantities, but of the mean of the quantities.

Chart 39 (page 273) should be headed "Perfect Inverse Correlation" instead of "Perfect Direct Correlation."

The last part of the text, which deals with sources and collection of statistics, outlines the steps taken in the planning of a statistical investigation, discusses the library method, the field method, and the office or correspondence method of obtaining data, and gives directions for editing schedules. It also discusses and lists the more important periodic statistical publications, and gives "a classification of economic and sociological data, with a partial bibliography of the sources of statistical information on each class of facts mentioned." Of particular value is the attention which is paid to some of the recently inaugurated publications which "may not be familiar to the average reader."

Following the text are these appendixes:

Appendix A. The Literature of Statistical Methods. This is a classified bibliography.

Appendix B. Data Tables, chiefly for use in connection with Appendix C.

Appendix C. Exercises and Study Questions, which apparently provide a good set of laboratory problems.

Appendix D. Aids to Computation. Here are given tables of squares, square roots, logarithms, and sums of squares.

The book is well indexed, and in additon to a table of contents contains lists of charts and plates, and of tables, symbols, and formulas. At the end of each chapter are given references.

The press work is good, the figures are an excellent example of what statistical

graphs should be, the typography is good, although in one or two instances the displayed work might be improved upon. Errors are infrequent. The pages present a pleasing appearance and are quite readable. The book is readable, too, in another sense, and should also be teachable. The material is well arranged and the order of subjects is good. In the opinion of the reviewer Professor Jerome has produced a good book, one from which can be obtained a clear insight into the most frequently used methods of statistics.

PAUL R. RIDER

Washington University
St. Louis

RECENT LITERATURE

STATISTICAL BOOKS

ADAMS, ARTHUR B. *Economics of business cycles.* New York: McGraw-Hill. 1925. xvi, 268 pp.

ATHEARN, WALTER S. and others. *Measurements and standards in religious education.* New York: Doran. 1924. x, 532 pp. (The Indiana Survey of Religious Education. Volume 2.)

BABER, R. E. and E. A. ROSE. *Changes in the size of American families in one generation.* Madison: Wisconsin. 1924. 99 pp. (University of Wisconsin Studies in the Social Sciences and History. No. 10.)

BERRIDGE, WILLIAM A. and others. *Purchasing power of the consumer. A statistical index.* Chicago: Shaw. 1925. xxv, 318 pp.

COOLIDGE, J. L. *An introduction to mathematical probability.* New York: Oxford University Press, American Branch. 1925. xii, 215 pp.

GOLDENWEISER, E. A. *Federal reserve system in operation.* New York: McGraw-Hill. 1925. x, 349 pp.

HEXTER, MAURICE B. *Social consequences of business cycles.* Boston: Houghton Mifflin. 1925. xxiii, 206 pp.

KATZENELLENBAUM, S. S. *Russian currency and banking, 1914–1924.* London: King. 1925. x, 198 pp.

KOHLER, ERIC L. *Accounting principles underlying Federal income taxes, 1925.* Chicago: Shaw. 1925. xiii, 550 pp.

LOMAN, H. J. *The insurance of foreign credits.* Philadelphia, 1923. 142 pp.

MCGRANE, R. C. *The panic of 1837. Some financial problems of the Jacksonian era.* Chicago: University of Chicago Press. 1924. vii, 260 pp.

NATIONAL INDUSTRIAL CONFERENCE BOARD. *The growth of works councils in the United States. A statistical summary.* New York, 1925. v, 15 pp.

——. *Tax burdens and public expenditures.* New York, 1925. vii, 70 pp.

——. *The tax problem in West Virginia.* New York, 1925. viii, 235 pp.

——. *Technical education and the textile industry. A summary report of an investigation.* New York, 1925. 13 pp.

——. *Wages and hours in American industry.* New York, 1925. 199 pp.

PECK, HARVEY W. *Taxation and welfare.* New York: Macmillan. 1925. vii, 269 pp.

ROSS, F. A. *School attendance in 1920. An analysis of school attendance in the United States and in the several states, with a discussion of the factors involved.* Washington, 1924. xx, 285 pp. (U. S. Bureau of the Census. Census Monograph V.)

STOCKDER, A. H. *German trade associations; the coal Kartells.* New York: Holt. 1924. iv, 254 pp.

THURSTONE, L. L. *The fundamentals of statistics.* New York: Macmillan. 1925. xvi, 237 pp.

VALGREN, VICTOR N. *Farmers' mutual fire insurance in the United States.* Chicago: University of Chicago Press. 1924. ix, 186 pp.

GOVERNMENT PUBLICATIONS

UNITED STATES.

—— Department of Agriculture.

Bureau of Agricultural Economics:

"Handbook of Instructions for Standardizing Tables." March, 1925. 40 pp.

—— Department of the Interior.

Bureau of Education:

No. 28 "Fiscal Report of State Universities and State Colleges." 1925. 165 pp.

—— Department of Labor.

Children's Bureau:

No. 140. "Child Dependency in the District of Columbia." 1924. 160 pp.

No. 143. "Child Management." 1925. 24 pp.

—— Treasury Department.

Public Health Reports:

No. 7. "Study of Morbidity in a General Population Group." February, 1925.

No. 9. "Death Rates in a Large Group of Insured Persons, 1924." February, 1925.

No. 11. "The Pan American Sanitary Code." March, 1925.

No. 12. "No transferable Immunizing Substances in Blood of Drug Addicts." March, 1925.

No. 16. "Current World Prevalence of Disease." April, 1925.

COMMONWEALTH OF PENNSYLVANIA.

Department of Labor and Industry.

Special Bulletin. "Union Scale of Wages and Hours of Labor, 1919–1924." 1924. 119 pp.

ARTICLES IN PERIODICALS

INTERNATIONAL LABOR REVIEW.

February, 1925. D. Papa: "The Adaptation of Wages to the Cost of Living in Hungary"; H. Dersch: "The Legal Nature and Economic Significance of the German Works Councils"; E. C. Snow: "The Practical Interpretation of Index Numbers"; T. Voionmaa: "Alcoholism and Industrial Accidents"; J. A. Dale: "Labour Problems in Northern Ireland."

March, 1925. K. Pribram: "The Unification of Social Insurance"; B. M. Stewart: "An American Experiment in Unemployment Insurance by Industry"; M. Gottschalk: "The Problem of Workers' Control in Belgium"; P. H. Douglas: "Some Precedents for the Family Wage System"; A. C. Maher: "Women Trade Unionists in the United States."

JOURNAL OF THE ROYAL STATISTICAL SOCIETY.

January, 1925. G. Udney Yule: "The Growth of Population and the Factors which Control it"; "Discussion on the Laws Governing Population"; A. A. Tschuprow: "On the Asymptotic Frequency Distribution of the Arithmetic Means of n"; J. Brownlee: "On the Error in the Correlation Due to Random Sampling when Proportionate Mortalities are Used."

METRON.

February, 1925. Georges-Risler: "Les berceaux vides"; A. Siegfried: "L'Expérience travailliste et le Probleme social en Angleterre."

March, 1925. Cuvelette: "L'habitation auvriere et les oeuvres sociales aux Mines de Lens."

GIORNALE DEGLI ECONOMISTI.

January, 1925. C. Bresciani-Turroni: "Considerazioni su Alcune Recenti Esperienze Monetarie"; U. B. Amoroso: "A Proposito di Meccanica Economica"; G. de F. Gerbino: "Frabrizio Natoli."

February, 1925. F. Vinci: "Le Illusioni Statistiche"; I. Messina: "La Teoria Degli Accumuli Esposta in Modo Elementare."

JOURNAL DE LA SOCIETE DE STATISTIQUE DE PARIS.

February, 1925. F. Lede: "Etude statistique de la premiere dentition"; J. Girard: "Les Annuaires de l'Office central de Statistique de la Repulique polonaise et la population de la Pologue"; M. Ricard: "Chronique des banques et questions monétaires."

March, 1925. J. Rueff: "Sur une théorie de l'inflation."

April, 1925. F. Lede: "Etude statistique de la premiere dentition (1885–1910)" (concluded); E. Michel: "Les Evaluations de la fortune privée en France depuis 1850 et la valeur actuelle de la propriétè immobiliere"; D. de Bernonville: "Chronique des questions ouvrieres et des assurances sur la vie."

MONTHLY LABOR REVIEW.

February, 1925. "Purchasing power of farmers' incomes"; "Wage scales in the building and printing trades"; "Food defects among industrial employees"; "Coöperative legislation in the United States"; "Strikes and lockouts in the United States."

March, 1925. "Accident-prevention plans of the Illinois Department of Labor"; "German automobile industry"; "Recommendations of the Presidents Agricultural Conference"; "Railroad labor accomplishment, 1922 and 1923"; "The coöperative movement: A selected bibliography."

April, 1925. "Trend of Employment of men and women in specified industries"; "Labor recommendations in Governors' messages of 1925"; "Promotion of standards"; "Employee's representation in coal mines and steel works"; "Wages and Hours in municipal employment in Massachusetts"; "Comparison of workmen's compensation laws as of January 1, 1925."

POLITICAL SCIENCE QUARTERLY.

March, 1925. R. L. Schuyler: "The Constitutional Claims of the British West Indies"; W. G. H. Cook: "Electoral Reform and Organized Christianity in England, II"; S. A. Rice: "The Behaviour of Legislative Groups"; E. M. Earle: "The New Constitution of Turkey"; T. R. Powell: "Umpiring the Federal System, 1922–1924."

Supplement: "Record of Political Events."

THE JOURNAL OF POLITICAL ECONOMY.

February, 1925. E. Abbott: "English War Statistics of Pauperism"; W. T. Morgan: "Labor's Attempt to Govern Great Britain"; P. H. Douglas: "Amount and Nature of Family Allowances"; E. Frankel: "Present-Day Tendencies in German Socialism"; J. A. Estey: "Stabilizing Agricultural Prices"; C. L. Benner: "Farm Depression and Credit, 1920–21. I"; J. Viner: "Tests of Competitive Prices of Cement."

April, 1925. H. L. Lutz: "Dividends in Income Taxation"; W. W. Hewitt:

"The Concept of Income"; L. K. Frank: "Industrial Integration"; A. H. Hansen: "Prices and Agriculture"; C. L. Benner: "Farms Depression and Credit, 1920–21, II"; A. Goldenweiser: "Banking Reform in Russia."

THE JOURNAL OF ACCOUNTANCY.

March, 1925. J. T. Anyon: "Early Days of American Accountancy"; W. B. Franke: "College and University Accounting"; S. Farb: "Insurance-brokerage Accounting."

April, 1925. L. J. Wolfe: "Use and Occupancy Insurance: Its Relation to the Accounting Profession"; C. W. McDowell: "Examination of Bituminous Coal Companies"; J. O. McKinsey: "Modern Tendencies in Accounting Practice"; J. P. Goddard: "Accountancy and the Tax Problem."

JOURNAL OF THE INSTITUTE OF BANKERS.

February, 1925. M. S. Herries: "Overseas Commercial Credits."

JOURNAL OF THE INSTITUTE OF ACTUARIES.

March, 1925. C. Carpmael: "The Value of a Deferred Annuity with special reference to the Valuation of Mineral Property"; "The Tercentenary of Common Logarithms."

THE ANNALS.

March, 1925. "Giant Power: Large Scale Electrical Development as a Social Factor."

MISCELLANEOUS REPRINTS AND PAMPHLETS

SOLUTION D'UN PROBLEME DU CALCUL DES PROBABILITEE, by Ragnar Frisch. Särtryck ur Skandinavisk Aktuarietidskrift, 1924.

ÜBER KURVEN ENDLICHER ORDNUNG and ÜBER ZERLEGBARE KURVEN, by Otto Haupt. Reprinted from Band 19, 1924 and Band 22, 1925, of Mathematische Zeitschrift.

BERMERKUNG UBER DIE EBENEN ELEMENTARKURVEN 3. ORDNUNG, by Otto Haupt. Reprinted from Band 92, 1924, of Mathematische Annalen.

ÜBER DIE TRANSFORMATION LIOUVILLE'SCHER MANNIGFATIGKEITEN, by O. Haupt and E. Hilb. Reprinted from Nachrichten der K. Gessellschaft der Wissenschaften zu Gottingen Mathematisch-physikalische Klasse, 1924.

ÜBER ANALYSIS SITUS, by H. Tietze. Reprinted from Mathematische Einzelschriften, 1923.

ÜBER DIE *Gauss-Green-Stokes* SCHEN INTERGALSATZE, by H. Tietze. Reprinted from the Journal für die reine und angewandte Mathematik, Band 153.

COST OF LIVING SURVEY. Reed College Bulletin, Vol. 4, No. 1. January, 1925.

OTHER BOOKS

BRECKINRIDGE, S. P. *Family welfare work in a metropolitan community. Selected case records.* Chicago: University of Chicago Press. 1924. xvii, 938 pp.

BROOKINGS, ROBERT S. *Industrial ownership: its economic and social significance.* New York: Macmillan. 1925. x, 107 pp.

JAGTIANI, H. M. *The role of the state in the provision of railways.* London: King. 1924. xi, 146 pp.

KEEN, F. N. *The law relating to public service undertakings, comprising a survey of the legal position of such undertakings generally, and a more detailed consideration of particular classes of undertakings.* London: King. 1925. xii, 320 pp. (London School of Economics. Studies. No. 79.)

KNEELAND, N. *Cases in retail salesmanship.* Chicago: Shaw. 1924. xv, 189 pp.
——. *Hosiery, knit underwear, and gloves.* Chicago: Shaw. 1924. xx, 126 pp.
——. *Waists.* Chicago: Shaw. 1924. xxi, 149 pp.
LEFFINGWELL, W. H. *Office management. Principles and practice.* Chicago: Shaw. 1925. xxvii, 850 pp.
NATIONAL INDUSTRIAL CONFERENCE BOARD. *Uniform medical provisions for workmen's compensation acts in the United States.* New York. 1925. v, 28 pp.
Palgrave's Dictionary of Political Economy, edited by Henry Higgs. Volume 1. A-E. New edition. London: Macmillan. 1925. xvii, 924 pp.
RICHMOND, M. E. and F. S. HALL. *Child marriages.* New York: Russell Sage Foundation. 1925. 159 pp.
RINGO, F. D. *Girls' and juniors' ready-to-wear.* Chicago: Shaw. 1924. xxi, 160 pp.
SCOTT, J. W. *Unemployment, a suggested policy.* London: Black. 1925. 63 pp.
WILKINSON, W. J. *Tory democracy.* New York: Longmans. 1925. 315 pp. (Columbia University Studies in History, Economics and Public Law. No. 257.)